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WHY WE MEASURE PEOPLE.

SOME Anthropologists go about the country with little cards in their hands on which they jot a memorandum of the colour of the hair and eyes of the people they come across. Others, with simple appliances, measure the stature and the span, the length and breadth of the heads, of the faces and of the noses of those who will submit to it. Many are the devices adopted and varied are the arguments employed to induce the country folk to allow themselves to be measured. Generally it is best to commence with the stature, as if it were wanted merely to find out who were the tallest men; most people take a pride in having large heads, and so vanity paves the way for the cephalic measurements. The subject then becomes interested and amused and the onlookers indulge in mild chaff, so that by one means or another not only can the above-mentioned measurements be taken, but others may be added. When the eye and hair colours and other physical features are noted, a very fair documental description of the individual has been secured. The subject is generally ready enough to be photographed, both full-face and side view, and a promise of a copy of his photograph will usually induce a recalcitrant person to submit to the entire operation.

The question not unnaturally presents itself to onlookers —Why are these observations and measurements made?

Now there are several reasons why people are measured. In most anthropometric laboratories the object is mainly to test the growth and physical fitness of people, so that they may see how they stand in relation to other people, and to

discover certain physical imperfections. M. Alphonse Bertillon, some twenty years ago, devised an excellent series of measurements for the identification of criminals. It will be obvious that a precise and rapid method of identification not only expedites justice and saves expense, but at the same time it is a safeguard to the prisoner, as it prevents his being punished for the crimes of others. It is really beautiful to see the celerity and certitude of the Bertillon system, and it is satisfactory to think that it is now, though tardily, being adopted in the British Islands. This system of identification is one of the practical applications of anthropology to ordinary life, and its utility is beyond question.

Another reason for measuring people is to endeavour to trace their racial characters and affinities. This is why peripatetic observations are made, and why an influential committee has been appointed by the British Association for the Advancement of Science to conduct an Ethnographical Survey of the United Kingdom.

The work done by this committee will be found in the Reports of the Association, but as yet no systematic survey of the British Islands has been attempted. The Ethnographical Survey of Ireland has been undertaken by a Dublin committee, which is supported by the Royal Irish Academy, and four comprehensive reports[1] have been published by that body. These reports are drawn up on the following lines:—

I. *Physiography of the district investigated.* II. *Anthropography.*—1. Methods; 2. Physical characters with lists of measurements; 3. Vital Statistics (general and economic), (A) Population, (B) Acreage and Rental, (C) Language and Education, (D) Health; 4. Psychology; 5. Folk names. III. *Sociology.*—1. Occupations; 2. Family Life and Cus-

[1] "The Ethnography of the Aran Islands, County Galway," by. Prof. A. C. Haddon and Dr. C. R. Browne, *Proc. Roy. Irish Acad.* (3rd ser.), ii., 1893, pp. 768-830, pls. xxii-xxiv; "The Ethnography of Inishbofin and Inishshark, County Galway," by Dr. C. R. Browne, *l. c.*, iii., 1894, pp. 317-370, pls. viii., ix.; "The Ethnography of the Mullet, Inishkea Islands and Portacloy, County Mayo," by Dr. C. R. Browne, *l. c.*, iii., 1895, pp. 587-649, pls. xv.-xvii.; "The Ethnography of Ballycroy, County Mayo," by Dr. C. R. Browne, *l. c.*, iv., 1897, pp. 74-111, pls. iii., iv.

toms; 3. Food; 4. Clothing; 5. Dwellings; 6. Transport. IV. *Folk-Lore.*—1. Customs and Beliefs; 2. Legends and Traditions. V. *Archæology.*—1. Survivals; 2. Antiquities. VI. *History.* VII. *Ethnology.* VIII. *Bibliography.*

It will be evident that this is a somewhat ambitious programme, and although in many instances the information given on a particular subject is meagre, owing to the very limited time available for work in the field, it was considered best to keep to the general scheme in order to emphasise the fact that in all investigations of this kind the widest possible outlook must be kept.

The late Rev. Dr. Walter Gregor has accomplished some very good work for the committee, especially in the folk-lore and physical characters of the natives of Wigtownshire and Kircudbrightshire. This south-west corner of Scotland is ethnologically important, as it was the stronghold of the Picts, a people about whom very contrary opinions exist. A preliminary "Report on Folk-lore in Galloway, Scotland," by the Rev. Walter Gregor, LL.D., was published in the *Report of the British Association* for 1896 (p. 612), and an abstract of the measurements made on the Galloway folk will be printed in the *Report of the Association* for 1897. A little work has been done on the physical features of the inhabitants of some of the villages near Cambridge; the main results will also be published in the 1897 Report. Other investigations of a similar nature are now being made under the auspices of the committee.

It should never be forgotten that these researches owe their inspiration to the laborious investigations of the revered Dr. John Beddoe on the hair and eye-colour of the inhabitants of the British Islands. His results were published in 1885 in his *Races of Britain*, a book which is a mine of information. Dr. Beddoe also made numerous observations of a similar kind on the continent.

In 1883 the Anthropometric Committee of the British Association published their final report. The committee was appointed for the purpose of collecting observations on the systematic examination of the height, weight, span, chest girth, breathing capacity, strength, colour-blindness,

eyesight, colour of eyes and hair of the inhabitants of the British Isles. Several of these may be of interest, but they are of no special ethnological importance. This report was drawn up by Mr. C. Roberts and Sir Rawson W. Rawson. And it is illustrated by a few maps showing the distribution of height, weight and certain combinations of the colours of eyes and hair.

In order to gain definite information respecting the racial affinities of a people it is necessary to have other measurements than the above-mentioned. A happy example

FIG. 1. SKETCH MAP OF THE DISTRICT.

of the value of employing different sets of observations is afforded by the brilliant work done Dr. R. Collignon in his studies on the anthropology of France. His paper on the five Departments of Dordogne, Charente, Corrèze, Creuse and Haute-Vienne (*Mém. Soc. d'Anth.*, Paris, i. (3*e* sér.), 3*e* fascic., 1894) constitutes so instructive an example of the modern methods of Anthropological investigation and of the lessons to be learnt from them that an abstract of it will furnish the best answer to the question: Why are these observations and measurements made?

The region under consideration consists partly of the calcareous beds, and partly of primitive rocks of the Central Plateau of France; the limiting line between them is shown on map No. 1; to the east it passes into the mountainous mass of Auvergne. The five Departments which constitute this region are traversed from east to west by the gradually decreasing elevations of the Limousin Mountains, which serve as barriers between the three basins of the Dordogne—or rather of its right affluents the Dronne Isle, Vézère and Corrèze; of the Charente and of the left affluents of the Loire, the Vienne, Gartempe, Creuse and Cher. A line running roughly north and south, starting at the junction of Charente and Haute-Vienne and passing not far to the east of Périgueux, would separate the fertile district to the west from the poor lands to the east. At certain points in the latter, as in the Limousin, the valleys are rich, but the uplands are infertile, and produce only chestnuts and scanty cereals.

The physical features of the population studied by Dr. Collignon are mainly those of the conscripts for the xii[e] Corps d'armée who are recruited from these five Departments.

The characters are given in the order of importance that Dr. Collignon allocates to each.

CEPHALIC INDEX.

This index is the ratio of the breadth of the head to its greatest length, the latter being taken as 100. In dealing with skulls, anthropologists usually arrange the indices in three groups: (1) Dolichocephals, with an index of less than 75; (2) Mesaticephals, with an index between 75 and 80; and (3) Brachycephals, having an index of over 80. It is the practice of some anthropologists to deduct two units from the corresponding index of the living head so as to reduce the cephalic to the cranial index.

There is a tendency at present not to lay too much stress upon these purely empirical divisions and some

would raise the upper limit of dolichocephaly two or three units.

The following table gives the distribution of the cephalic indices in the five Departments, in the case of Dordogne a further analysis is made which proves that the southern part of that Department is much more brachycephalic than the northern. The mean index of this Department, if alone considered, gives extremely little information.

CEPHALIC INDEX—PROPORTION PER CENT.

	Charente.	Haute-Vienne.	Creuse.	Corrèze.	Dordogne.	North Dordogne.	South Dordogne.
67-69	·17				·22	·30	
70-74	5·60	3·78	·58		5·95	8·24	
75-79	41·95	37·81	19·24	6·90	38·30	48·53	11·54
80-84	41·82	42·86	62·38	43·27	39·46	37·63	44·22
85-89	10·85	14·71	16·92	41·73	13·83	5	39·92
90-94	·50	·84	·88	7·93	2·13	·30	6·93
95-97				·17	·11		·39
Mean index	80·43	80·93	82·16	84·93	80·70		

It is evident that this table indicates considerable differences in the ethnic constitution of each Department. Taking the extremes, we have on the one hand, North Dordogne with its 8·5 per cent. of indices below 75, or Charente with 5·7 and only ·5 of ultrabrachycephals and Corrèze on the other which has no dolichocephal below 75, but has 8·1 per cent of indices over 90.

Taken as they stand, the great majority of these indices fall into the brachycephalic division, while very few are dolichocephalic.

The mean index of the French population being 83·57, Dr. Collignon, in order to simplify matters, describes as brachycephals those indices above 83. The cantons which come under this grouping form a compact mass to the south and south-east as is seen in the accompanying map. To the north there are two islands in which the index does not exceed 83·8.

Inversely, and as a matter of convenience, he regards as dolichocephalic all the regions in which the index is less than 80. Two large groups of dolichocephalic cantons are isolated by this means; the more important covers two-thirds of the department of Dordogne (the valleys of the Isle and of the Dronne) and about one half of Charente, mainly to the south and south-east. The other has Limoges for a centre and the seven cantons that surround it.

In the narrow band of country between these two groups the index is 81.

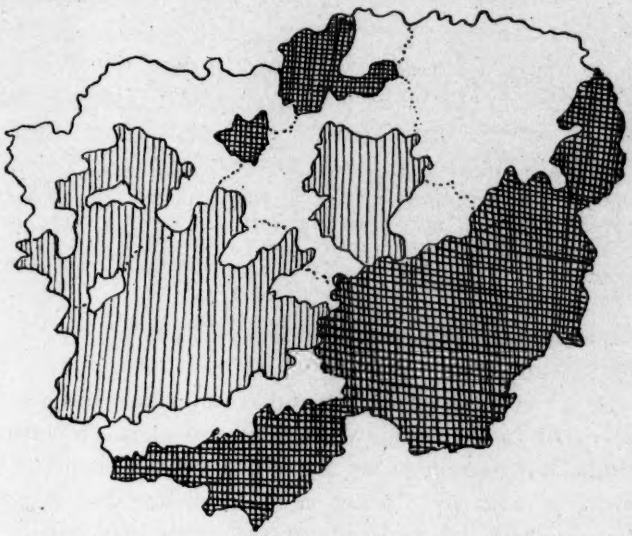

FIG. 2. THE DISTRIBUTION OF THE CEPHALIC INDEX.

Areas with an index of less than 80, shaded; those between 80 and 83, left blank; those over 83, cross-hatched.

This clearly defined distribution is of the greatest importance, for alone it provides a key to the local ethnography.

Another point not less worthy of attention is the clear manner in which these two head types are separated: 1. between the two Departments of Dordogne and Corrèze; 2. between the two portions of Dordogne which are separated by the rivers Vézère and Dordogne.

As a matter of fact the boundary between the two

Departments of Dordogne and Corrèze was formerly precisely that between Périgord and Limousin, and in earlier times between the Petrocorii and Lemovices. To the right of this entirely conventional frontier the indices run from 85·4 to 87·3, while to the left they vary from 78·7 to 81·4, but there is nothing in history to explain this discrepancy. The explanation appears to be that well before the Conquest the two peoples differed in race, the one being what Cæsar called Celts, the other probably belonging to the people whom he named Aquitainians.

The southern portion of Dordogne is also brachycephalic and Celtic, and so Dr. Collignon is inclined to think that it did not form part of the territory of the Petrocorii, but that it should be divided among the Nitiobriges and Cadurci, whose equally brachycephalic descendants still people Lot-et-Garonne and Lot. Another line of evidence supports this conclusion. It is known that the primitive episcopal dioceses corresponded to the territories of the ancient Roman *civitates*, since a bishop was established in each city by the emperors. Whilst the northern, eastern and western frontiers of the diocese of Périguex correspond very closely with that of the modern Department, the region south of the Vézère belongs to the Bishop of Cahors, which tends to show that the natives of the south of Dordogne are the descendants not of the Petrocorii, but of the Cadurci.

The differences between the two parts of Limousin, of which the one forms part of Corrèze and the other the south of Haute-Vienne, can be explained in an analogous manner. The former is brown and brachycephalic, while the latter is fair and dolichocephalic.

One may well believe that the Lemovices, those of the neighbourhood of Limoges, were no more Petrocorii than Celtæ, but a fair people of Belgic or Germanic origin, established in Celtica, who had over-lorded the ancient brachycephalic people who there preceded them.

Inversely, Briva-Curetia, another old Gaulish town of Limousin, was the centre of gravitation of the first inhabitants, if not their capital.

In Charente there is only one canton in which the mean index rises over 83. In this canton of Chabanais is the small village of Chassenom on the left bank of the Vienne. It is interesting to see the old Celtic race here, preserved with a relative purity, still grouped around the ruins of its oppidum (Cassinodunum), where, compared with the rest of the Department, it appears as an island surrounded by the combined flood of brown and fair dolichocephals.

COLOUR OF THE HAIR AND EYES.

A statistical inquiry concerning the distribution of the colours of the eyes and hair leads to the following results. The browns predominate markedly over the blonds. But for a group of cantons in Creuse all the district should be ranged under the brown or moderately brown categories.

In the following table the numbers are in relation to 100, the differences between 100 and the fairs and the darks represent the eyes and hair of intermediate tint :—

	Eyes.		Hair.			Half sum of eyes and hair.		Excess of dark over light.
	Blue.	Dark.	Fair.	Dark and black.	Black only.	Light.	Dark.	
Haute-Vienne	36·7	24·6	21·8	49·6	5·25	29·2	37·1	7·9
Creuse - -	34·7	23·3	21·9	53·9	6·12	28·3	38·6	10·3
Charente -	33·8	23·6	17·2	57·6	5·80	25·5	40·6	15·1
Corrèze -	29·5	23·3	15·4	58·4	3·80	22·3	40·9	18·6
Dordogne -	34·2	23·6	15	66·3	12·05	24·6	45·0	20·4

On comparing this table with the map it will be seen that although Dordogne has an absolute greater number of blonds than Corrèze it is relatively darker, owing to the fact that the darks are greatly in excess in certain cantons; in other words, Dordogne is more patchy and Corrèze more uniform in the distribution of their hair and eye colours. It is evident in using the word blond this term is employed in only a relative sense. It is with this reserve and for the sake of convenience that the term blond will be employed. In the most blond group, that in the neighbourhood of

Aubusson in Creuse, the blonds amount to only 33·6 per cent.—that is to say, one-third.

In order to gain a clear conception of the distribution of the hair and eye colours, it will be simpler to assume the whole region as originally inhabited by a brown population and then to follow the probable route of the blonds.

The most important spot where the blond type is best preserved is the east of the Department of Creuse, especially the plateau of Gentioux and the upper basin of the river Cher and of its left affluents.

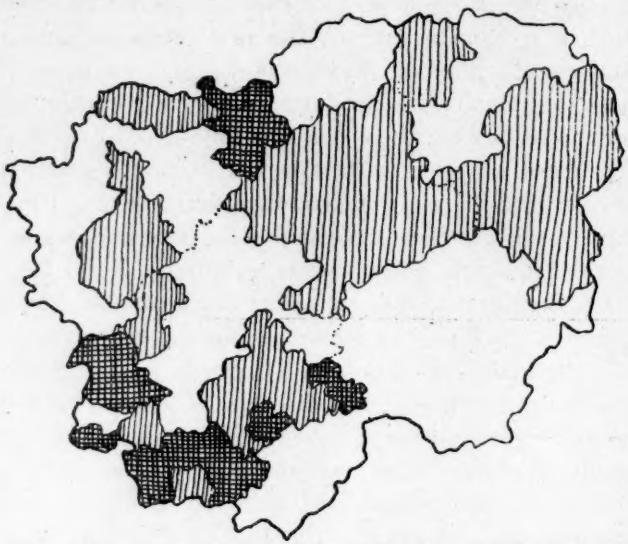

FIG. 3. THE DISTRIBUTION OF COMBINED HAIR AND EYE COLOUR.
Excess of Browns from 0 to 10, shaded; 10 to 30, blank; over 30, cross-hatched.

The second relatively blond region has Limoges for its centre. In certain spots the type is preserved with a remarkable purity, particularly among the women. Dr. Collignon was very much struck with the resemblance of these to the women of Contentin in Normandy. It appears that the blonds radiate from Limoges in four directions: 1, towards the north in the direction of the old Roman road of Argentomagus and Avaricum (Argentan and Bourges), later the route to Paris, that is to say, along the road which united this town with the great blond centres

of the North of France; 2, towards the east where it joins with blonds of the Cher region; 3, to the west in the direction of Angoulème; and 4, southwards towards Périgueux.

The third route of blond immigration would be the route from Paris to Bordeaux through Angoulème.

Limoges formed a centre, and towards the four points of the compass lay four very ancient and important towns, Avaricum (Bourges), Gergovia (Clermont), Vesuna (Périgueux), and Ecolisma (Angoulème). The latter town was the only one of the four that was not united to Limoges either by a Roman road of the first order, or later by a postal route; and we find in the region between these towns the blonds are deficient. The importance of the communications between Limoges and Bordeaux through Périgueux is affirmed by the long line of blonds which occur along that route. To take a biological simile, Limoges represents a ganglion protruding its nerve fibres in all directions towards other similar ganglia.

The distribution of black hair is worthy of note. In Dordogne it is marked in *la Double*, in the valleys of the rivers Dordogne, Isle and Dronne. Secondary centres extend towards the north of Charente and of Creuse. There is thus a current inverse to that of the blonds. The great pressure of blonds came from the north-east and from the north; it traversed the district obliquely in a north-east to south-west line. Inversely the black-haired race appears to be massed in the south-west, and to be distributed, with a gradually decreasing importance, towards the north-east and north.

STATURE.

The measurements of the stature are not so instructive from a racial point of view as might have been expected.

All the tall statures are massed at the circumference of the four Departments of which the statistics are available, with the exception of an important centre about Limoges. In the map the distribution of the heights over 1640 mm. (5 ft. $4\frac{1}{2}$ in.) is shown by the vertical lines. In mapping

the distribution of the statures under 1610 (5 ft. 3½ in.) it is seen that besides several scattered areas towards the south of the district under discussion there is a large central area, which, following the example of Broca who found a similar area of a dwarfed population in Basse Bretagne, Dr. Collignon calls "the Limousin black spot" ("*la tache noire limousin*").

In other cantons less than 10 per cent. of the statures have under 1600 (5 ft. 3 in.), those in the black spot have

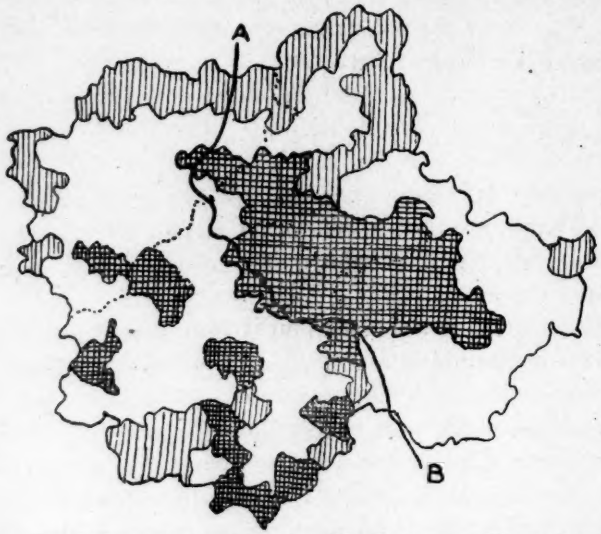

FIG. 4. THE DISTRIBUTION OF STATURE. (The Department of Creuse is omitted.)

Stature less than 161 cm. (5 ft. 3⅜ ins.), cross-hatched; between 161 and 164 cm., blank; from 164 (5 ft. 4½ ins.) to 166 cm. (5 ft. 5¼ ins.), shaded.

The line AB separates the granites and crystalline rocks on the east from the calcareous beds on the west.

without exception over 30 per cent.; eight cantons have more than 40 per cent., one has 54·7 per cent. while that of Saint Mathieu has 67·6 per cent. less than 1600, four below 1540, and 8·8 per cent. below 1500 (4 ft. 11 in.)! True dwarfs, that is those with a stature below 1500 mm., are exceptional everywhere.

These figures are not due to an accidental and temporary selection as the following figures of Bondin prove, which extended over a period of thirty years (1831-1860). These

tables show that Dordogne, Corrèze and Haute-Vienne are among the four Departments in the whole of France which have the greatest number of exemptions from conscription owing to deficiency of stature. The neighbouring Department of Puy-de-Dôme occupies the eighty-fourth rank with 128 exemptions.

	Exemptions per 1000.	Rank in France.		Heights of 1732 per 1000.	Rank in France.
Creuse - -	89	63	Creuse - -	44	74
Charente -	114	82	Corrèze - -	43	77
Dordogne -	124	83	Charente -	41	79
Corrèze - -	168	85	Dordogne -	39	80
Haute-Vienne	175	86	Haute-Vienne	31·6	86

Inversely the high statures are also at a minimum. Haute-Vienne having the least proportion of tall people and the greatest population of short people of any Department of France.

Bondin and Broca considered that this remarkable shortness was purely a question of race, the normal smallness of the brachycephals. This very simple explanation will no longer suffice, in the presence of the dolichocephaly proved for Dordogne, Charente and Haute-Vienne. If we compare the maps of the distribution of the cephalic index with those of colour and stature, and mentally superimpose them, we find that there is absolutely not a shadow of a relation between them. The "black spot" extends alike over the brachycephals of Corrèze, the brown dolichocephals of Dordogne, and the fair dolichocephals of Haute-Vienne. There is then no relation between this demonstrated phenomenon and race.

Some anthropologists seek a cause in the geological character of the soil; but here as in Brittany and Cotentin it explains nothing. It is true that the line of separation between the granites and crystalline rocks on the east and the calcareous beds on the west runs pretty closely along the southern border of the black spot; but we also find the greatest number of high statures on the granites and the

low statures flourish equally well on the Liassic and Cretaceous calcareous beds of Sarladais and Riberacois.

The only plausible explanation is the social condition, and in this case it is summed up in the expressive French term *la misère.* The steep slopes and barren soil only produce poor cereals, rye, barley and buckwheat. The natives live on these and on milk and chestnuts. Communication is difficult; no great tillage as in the fertile valleys of the Vienne and Gartempe, none of the larger industries that enrich a people. "In the cantons of Vigeois, Uzerche and Treignac in Corrèze" writes M. Vacher, "the population is settled in confined valleys, in deep gorges receiving little light and air, with an impermeable subsoil and marshy ground." In a poor country the most elementary hygiene is unknown, the death rate is raised and organic defects are more frequent than elsewhere. One of the more direct corollaries of misery is ignorance. In many other parts of France, as in the Hautes-Alpes and Sologne, poverty is allied with ignorance and results in the degeneration of the race.

THE NASAL INDEX.

The nasal index is the ratio of the breadth of the wings of the nose to its length, the latter being measured from the root of the nose to where the septum passes into the upper lip. The narrow noses (leptorhines) are those with an index below 70; the mesorhines range from 70 to 85; while the broad noses (platyrhines) are those above 85.

The mean nasal index is 68·8, but the individual range is enormous, 49·9 to 96·4, that is more than 46 units. As a whole the mesorhine indices, *i.e.*, those over 70 are massed in the centre of the five Departments.

This distribution follows in the main that of the stature. But why? Simply in accordance with a law previously thus formulated by Collignon: "In a given race, leptorhiny is in direct relation to stature; the higher this is raised the longer the nose, the lower the height the more the nose tends to mesorhiny".[1]

[1] "Etude anthropométrique élémentaire des principales races de France." *Bull. Soc. d'Anthrop.* Paris, 1883, p. 508.

A careful consideration of the data tends to show that, independent of stature, the brachycephals possess a mean nasal index of about 69, that is to say very near mesorhiny, which is in agreement with previous investigations. The dolichocephalic races are more leptorhine.

One result of this inquiry is that the value of the nasal index has received a serious blow. Certainly this character is very important for the discrimination of the great trunks of mankind, as has been abundantly proved in anthropological investigations in India, but so far as the European peoples are concerned, it is incontestable that the nasal index has only a subsidiary and relative value.

HEIGHT INDICES OF THE CRANIUM.

The importance of the vertical height of the cranium as a racial character has been emphasised by Virchow, but Collignon was the first to study this factor in the living. The two height indices are obtained by comparing the total height of the head measured from the vertex to the centre of the ear-hole with—(1) the length of the head, and (2) its greatest breadth, each of these two diameters being taken as 100.

The indices are classified as follows :—[1]

	Height-length Index.	Height-breadth Index.
Platycephals - - - -	- 67	- 83
Mesocephals - - - -	67 - 70	83 - 85
Hypsicephals - - - -	70 +	85 +

A really high skull, if it is very broad, may appear relatively low, or a low, but very narrow head, may appear decidedly hypsicephalic. Hence the necessity to consider first the cephalic index and thereby to recognise the normal and harmonic fluctuations of the inverse variations of these two vertical indices.

[1] These indices are taken from a subsequent memoir by Dr. Collignon. (*Mém. Soc. d'Anthrop.* Paris, i. (3e sér.), 1895, pp. 94, 95.)

Dr. Collignon has plotted the distribution of these indices for the Department of Dordogne alone. We have seen that the northern cantons are what he termed dolichocephalic, and the southern are brachycephalic. The length-height index of the former varies between 65 to 68, and of the latter from 70 to 72. Taking the mean at 66 and 70 respectively, it follows that the dolichocephals are platycephalic and the brachycephals hypsicephalic; but this platycephaly is a true flattening of the skull, and is not merely due to a lengthening of the cranium, as it is not the most dolichocephalic cantons that are the most platycephalic.

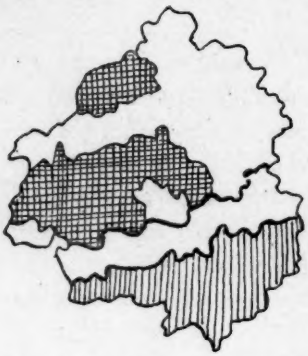

FIG. 5. THE DISTRIBUTION OF THE HEIGHT-LENGTH INDEX IN DORDOGNE.

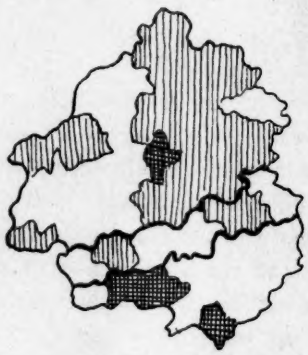

FIG. 6. THE DISTRIBUTION OF THE HEIGHT-BREADTH INDEX IN DORDOGNE.

70 +	Hypsicephalic (shaded)	85 +
67 – 70	Mesocephalic (blank)	83 – 85
– 67	Platycephalic (cross-hatched)	81 – 83

The oblique band enclosed with a thick line corresponds to the division between the dolichocephals and brachycephals (see fig. 2).

On the other hand, all the brachycephalic cantons have a height-breadth index of from 81 to 84, that is they are, or appear to be, platycephalic and mesocephalic, but their mean is mesocephalic.

The mixed race who inhabit the zone between the brachycephals and dolichocephals (cephalic index 80-82) is also intermediate with a height-breadth index of 83-85, but the dolichocephals fall into two groups, the one with indices from 85 to 87 are hypsicephalic, the others, like the brachycephals, are mesocephalic and platycephalic.

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Thus the platycephaly of the valley of the Isle is established.

The brachycephals are only false platycephals owing to an exaggeration of the transverse diameter.

Without going into further details, we may now make an attempt to unravel the ethnology of these five Departments. Taking the three characters of cephalic index, colour and stature, we can distinguish: short and dark or tall and fair brachycephals; fair tall dolichocephals and dark dolichocephals.

The brachycephals occupy all the region south of the rivers Dordogne and Vézère, the whole of the Department of Corréze and the east of that of Creuse. The brown brachycephalic type extends to the mountainous region of Auvergne, to the East of France and to the South of Germany. This race of short, dark brachycephals is a well-marked type which has received several names. Dr. Collignon, for want of a better term, adopts Broca's designation of Celts, as the founder of French Anthropology considered that these were essentially the Celtæ of Cæsar. They are often called Auvergnats. The tall fair variety is due to a crossing of this type with the fair race. A similar racial mixture occurs in Lorraine.

The fair dolichocephals inhabit the upper valley of the Cher; the neighbourhood of Limoges, whence they spread to the south following the plateaux that separate the valleys of the Isle and of the Dordogne; and also the north of Charente, Angoulême, and in general along the very ancient route between Paris and Bordeaux. These are the modified descendants of the tall, fair, dolichocephalic race of North Europe. Dr. Collignon speaks of it as the Hallstadt race.

The brown dolichocephals require a further analysis.

1. A type can be distinguished which is characterised by its relative platycephaly, the extreme broadening of the face, a prominent chin, low orbits and by the dark colour of the skin and hair. As it is usual in Europe to correlate a long narrow face with a long head, and a short, broad face with a rounded head, the association, as in this case, of a long

head with a broad face forms what is termed a hisharmony. In the fair dolichocephals, on the other hand, the head is high, the face narrow, the chin moderately prominent, the orbits normal, the skin, hair and eyes fair. It is obvious that these two races are entirely distinct.

2. A narrow-faced dolichocephal with a high head can be distinguished, but Dr. Collignon believes that it is a cross between two races, the brown and the fair dolichocephals. This is a very favourable combination, and gives rise to a beautiful variety of man.

3. A rare but recognisable type, with an extraordinarily narrow and elongated face, a retreating forehead, projecting jaws and retreating chin, the concave nose is so broad as to be nearly platyrhine, the hair and skin are dark.

Putting the second of these two varieties out of count, there only remain the brown dolichocephal with a dysharmonic face, and that with a retreating chin. They both live in the basin of the Isle and its affluents, as much in Charente as in Dordogne.

From numerous other investigations we know that the Neolithic dolichocephals of Western and Southern Europe were a slight people with brown hair. They constitute the Mediterranean race of Sergi, the western branch being generally termed Iberians. The ancient cave-men of France belonged to the same race; by comparing certain indices of these with the first group of our brown dolichocephals we find a remarkable correspondence :—

	Cephalic index.	Height-length index.	Height-breadth index.
Caverne de l'homme mort -	Dolicho.	Platyceph.	Mesoceph.
Old man of Cro-Magnon -	,,	,,	Platy.
Recent Dordogne - -	,,	,,	Platy. & Meso.

Further, the Cro-Magnon man had a dysharmonic face, this is also characteristic of the Neolithic dolichocephal of Laugerie, and it survives in their descendants in the valley of the Isle.

The remaining brown dolichocephalic type, with its low-

typed, long, narrow, prognathous face, is considered by Dr. Collignon to be the far-removed descendants of the Quaternary race of Canstadt and Spy. The same type has been recognised by him in Tunis among the Berbers of Djerid (Collignon's "*type du Gétule*") as well as in Dordogne and in the south of Charente, that is to say, in places still occupied by the descendants of the race of Cro-Magnon. It might be expected that the very ancient race of Canstadt, and the later race of Cro-Magnon, were together beaten back by the great pre-historic invasions of Western Europe.

A few words will suffice to trace the pre-historic settlements and racial movements that have occurred in this district.

The earliest inhabitants were probably the people with retreating chins. According to the opinion of Dr. Collignon these were kinsmen to Palæolithic man. At the present day, as is only to be expected, this type is very rarely met with in anything like purity, and it is very difficult to isolate it statistically.

The whole west of Europe was later occupied by the brown dolichocephals, the Iberian branch of the great Mediterranean race, of which the Cro-Magnon man was a variety. They buried their dead in the caves of the valleys of the Vézère, Isle and Dronne. Judging from their art, they were a skilful people and not devoid of culture :—

> Later he pictured an aurochs—later he pictured a bear—
> Pictured the sabre-toothed tiger dragging a man to his lair—
> Pictured the mountainous mammoth, hairy, abhorrent, alone—
> Out of the love that he bore them, scribing them clearly on bone.

There, protected in their barren, rocky valleys, weathering the storm of race conflict, unsubmerged by waves of race migration, still survive the children of early neolithic man.

Also in neolithic times a short, dark, brachycephalic folk came into France from the east by two routes flowing north and south of the Alps. The invasion followed the left bank of the Danube, entered the valley of the Rhine, and later

spread into France through the pass of Belfort and by the lower Moselle. A second, probably later and less important, invasion crossed the river to reach Upper Italy and Switzerland, and thence gained the valley of the Rhone. Thus their migration has been from east to west.

When the invasions came of the tall, fair dolichocephals: Kymri, Gauls, Cimbrians, Burgundians, Visigoths, Franks, etc., they more particularly followed a course parallel to the North Sea. From an ill-determined point to the north-east or north, they advanced invariably along the plains, probably on account of the chariots which they always brought with them. After having covered the plains of North Germany, where since then their descendants have always lived, and which became a second centre for emigrations, they passed to the north of the Black Forest to scatter upon the Netherlands and Flanders, the valley of the Seine and that of the Rhine. Thence their swarms were divided by the central plateau of France, one stream being diverted into Italy, the other into Spain and thence to North Africa.

The Roman conquest scarcely, if at all, affected the population of these five Departments, and it is more than certain that since then no foreign element has produced any result that can be traced, for all the Barbarians, as well as the English, belonged to the fair race.

In a subsequent memoir on the Anthropology of the South-West of France (*Mém. Soc. d'Anthrop.*, Paris, i., 3^{e} sér., 4° fascic., 1895) Dr. Collignon sums up his conclusions as follows:—

Such is, after an examination of anatomical characters, the distribution of the races in the south-west of our country. Is it possible to draw therefrom reliable indications of what it was formerly? Regarding this we may lay down this rule. When a race is well seated in a region, fixed to the soil by agriculture, acclimatised by natural selection and sufficiently dense, it opposes, for the most precise observations confirm it, an enormous resistance to absorption by the new comers, whoever they may be.

The most striking example of this stability of seated races, of this force of inertia which renders them victorious,

is certainly presented to us by Egypt. The modern Fellah differs in nowise from his ancestors, several millenniums ago who lived at the times of Tothmes and Rhameses, although, according to the calculations of M. Hamy, slavery had introduced upon the borders of the Nile more than 20 millions of negroes. These, in a climate which at first sight would be favourable to their acclimatisation, were not able to perpetuate their race, neither directly nor indirectly, that is to say, by crossing. All the more reason, one may say, that the same can be said of the historic conquerors of this unfortunate country, from the Hyskos and the Persians up to the Turks and the latest comers, the English. The waves of foreign blood that have spread over Egypt have disappeared never to return.

The reasons are many. If the aboriginal race is more numerous than its invaders, and this is nearly always the case, it can not be entirely destroyed, whatever be the slaughter which accompanies the conquest; the women and the children are preserved. The importance of the subsequent crossings can not then, at the maximum, attain more than one-third. The stable condition that follows puts then *ipso facto* the new comer in a minority from the commencement of the conquest, the work of selection by acclimatisation does the rest. It is a matter of a few generations.

The only case where the occupation can be definitive is that of an invasion by a very superior race emigrating with women and children to a region peopled by nomads or true savages,—such as the occupation of the United States or of Australia by the Europeans. In Canada, despite the political occupation and the incessant arrival of emigrants of their own blood, the English are absolutely balanced by the old French element, who were masters of the soil before their arrival.

But the presence of woman at the time of a conquest, if she is indispensable to a real and definitive colonisation, since alone it ensures the perpetuity of pure descendants, is not, however, sufficient. Except in a savage country, the women of the conquering party would always be in a

minority. Even in the case where restrictive laws would assure to their progeny particular privileges, making a kind of aristocracy, it could never happen that there would be only two strata of the population, a victorious aristocracy superimposed upon a conquered democracy. We know the fate of all aristocracies. Their grandeur is their ruin; they survive thanks only to foreign relays, and on an average disappear in three or four centuries. One cannot say "*Væ victis*," but "*Væ victoribus*," everything comes to him who waits.

The Romans did not systematically depopulate Gaul, her submission satisfied them; the distribution of races at the time of the Roman peace did not undergo other changes than those which could operate quite locally, the deporting of a too obstreperous people or colonising by veterans. The Barbarians passed like a torrent, they destroyed much, but they have not made in their campaigns a true colonisation "*ense et aratro*" of Marshall Bugeaud. The sword sufficed to assure their domination; to the vanquished—work. They have disappeared, except perhaps in the towns where they crossed with the Gallo-Roman middle class, after having preserved the forms or the imperial administration, for want of knowing and of being able to do better. The Arabs traversed the country, but to immediately disappear. It results, once more let it be repeated, that the present distribution of races should faithfully represent to us their ancient distribution, except in places where special economic conditions have been slowly modified, but in a constant manner, by foreign influences.

A. C. Haddon.

THE INFLUENCE OF OXFORD ON THE HISTORY OF GEOLOGY.[1]

ONE of the most immediate effects produced by contemplation of the world around us is that of pleasure in its abounding beauty. Whether we wander through the smiling country which surrounds this University, or climb the snowy peaks of the Alps, or visit the sunlit islands of the Tropics, wherever we turn, the feeling expressed by a German poet will spontaneously arise: "Oh! Wunderschön ist Gottes Erde, Und Schön auf ihr ein Mensch zu sein"—Beautiful is God's Earth and good it is to be a Man thereon.

This æsthetic delight, while it may sometimes suffice for the poet, is succeeded generally by a desire for closer acquaintance; a certain divine curiosity implanted in the breast of man leads him to search into the inner mysteries of Nature, and to explore the causes of the wonderful phenomena which surround him. He begins to examine and compare, to analyse the complex into its elements, and to build up again the elements into a new and intellectual cosmos. Thus he gains a new pleasure from contemplation of the world, and labouring in the pleasant work of investigation he experiences the joy of discovery.

The pleasures of sport are generally supposed to be those to which the soul of the Englishman is most deeply responsive. Thus the famous Darwin writing of his youth remarks: "The autumns were devoted to shooting . . . my zeal was so great that I used to place my shooting boots open by the bed-side when I went to bed, so as not to lose half a minute in putting them on in the morning. . . . How I did enjoy shooting!" But a little later, and he writes: "During the first two years my old passion for shooting survived in nearly full force, and I shot myself all the birds and animals for my collection; but gradually I gave up my

[1] An Inaugural Lecture delivered before the University of Oxford, on 19th October, 1897.

gun more and more, and finally altogether, to my servant, as shooting interfered with my work, more especially with making out the geological structure of a country. I discovered, though insensibly and unconsciously, that the pleasure of observing and reasoning was a much higher one than that of skill and sport."

This fragment of personal history seems to me even more impressive than the glowing words of Thierry, who although himself an invalid, wrote "Il y a au monde quelque chose qui vaut mieux que les jouissances matérielles, mieux que la fortune, mieux que la santé elle même, c'est le dévouement à la science!"

So much I have ventured to say in eulogy of all Science, of my own subject I would only add that it enjoys what is supposed to be a thoroughly English characteristic, it is fond of the open air; its best work can only be accomplished in the open air, it is there that its greatest triumphs have been won, and must continue to be won.

Many great discoveries have been made in Science by observers who did not consciously set out with a predetermination to discover something, but whose imagination was fired by some object or occurrence rather out of the ordinary course of Nature; as Bacon has observed: "It is probable that Prometheus, when he first struck the flint, must rather have marvelled at the spark than expected it". With the advance of Science we become less open to surprises, and the number of discoveries due to some happy chance constantly tends to diminish; such, on the other hand, are peculiarly characteristic of Science in its infancy. It would appear that the strange phenomenon which first directed attention to the problems of geology was the occurrence of curious stones, such as are now termed fossils, that, having a likeness to the hard parts of animals and plants, are yet found, to the surprise of the observer, lying embedded in the substance of rocks which form the interior of a country often at a great distance from the sea, and sometimes at a great elevation above it, as in the case of fossils in the Himalayas, which are obtained from that lofty mountain chain at a height of 16,000 feet.

No doubt the nature of fossils had awakened a lively interest in very early times. In Babylonia, that ancient mother of Arts and Sciences, many different kinds of fossils must have been familiar objects to the workmen who dug clay for bricks, or hewed stone out of the quarry; they were probably seen and pondered over by Chaldean philosophers, whose speculations concerning them were afterwards embodied in a mythical cosmogeny.

In ancient Greece they were well known and understood; but in Christian Europe, up to the sixteenth century, the civilised world was occupied with problems more important than the study of fossils; and even when in the revival of learning Fracastoro, Leonardo da Vinci, and Pallisy the potter, uttered true words concerning them, these were little heeded, and it was not till after the middle of the seventeenth century that fossils commenced to arouse that serious attention which with increasing earnestness has continued to be given to them ever since. It is in two little books, one entitled *Canis Carchariæ Dissectum Caput*, and the other *De Solido intra Solidum Naturaliter Contento*, published in 1669, that the germ of modern geology is to be found. They were written by the famous Steno, the true founder and father of our science. Steno was by birth a Dane, who occupied for a time the Chair of Anatomy at Padua. Dwelling thus not far from the shores of the Adriatic, he was able to make studies in marine zoology, and of these the most important was his dissection of a shark's head. Steno paid particular attention to its jaws and teeth, which are well worthy of study, as being probably the sharpest cutting instruments naturally produced. But these put him in mind of certain "glossopetræ" which are dug out of the ground in Malta, and on making a careful comparison of the fossil with the recent teeth he convinced himself of their precise similarity. But according to Steno's logic, nothing but a shark could make shark's teeth, and he consequently concluded that the ancient glossopetræ had once belonged to the mouth of a shark, and that the mouth opened into a shark's body behind it. You cannot assert, he remarks, that Nature will make the hand of a man with-

out the man himself, and the argument is just as true of sharks' teeth. Lest the great numbers in which the glossopetræ occur should be thought a difficulty—they were carried away from Malta in bushels—he points out that every shark's jaw contains sixty teeth in good working order, and these as they wear out are continually replaced by fresh ones; further, he adds, sharks swim about in schools, so that a great many teeth will generally be found bristling about in the same place at the same time. Since sharks are very voracious creatures, the multitude of glossopetræ implies the existence not only of sharks, but of a whole world of other animals, including those which live on cockles and other shell-fish. That we next find Steno giving serious attention to the study of cockle-shells is only therefore what we might expect. He patiently traced the manner of their growth, and ascertained, with a degree of accuracy remarkable for those times, the minute details of their structure. From the living cockle he turned to the fossil shells, and by a careful comparison showed how they agree, feature by feature, both in form and more particularly in structure, with their modern representatives. This structure, Steno insists, is the direct result of the manner in which the shell is produced by the animal, and he concludes that the fossil, like the recent cockle-shell, once contained a living cockle inside it.

Having thus, by close observation and strict logic, breathed life afresh into these fossil remains, Steno proceeded to provide them with an environment: they were bathed, he says, with an ambient fluid, which was none other than sea-water; hence it follows that the lands in which such fossils occur, were once submerged beneath the sea. This conclusion, he pointed out, is in complete harmony with the nature of the material of which the land consists, and in which the fossils lie embedded, for it resembles in the most striking manner the ooze of the sea-floor. In support of his argument Steno called attention to many interesting points of detail, such as the discovery of a fossil pearl-oyster, with a pearl still sticking to it: of fossil oysters perforated by galleries such as are produced by boring

worms; and of another fossil shell, covered with barnacles, which were adherent to its worn surface, a fact from which he drew several ingenious conclusions.

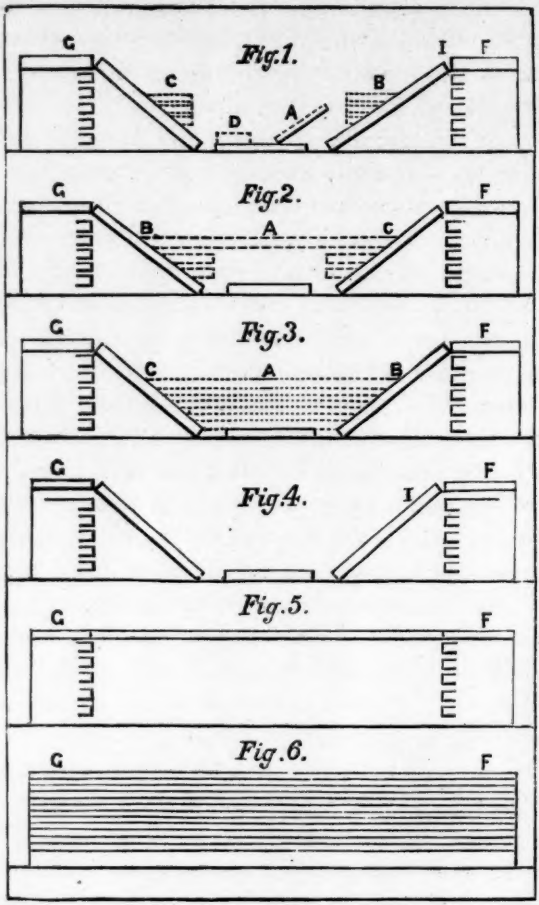
Returning to the sediments in which fossils lie buried, Steno pointed out that they are distributed in beds or strata, which are frequently horizontal; and he concluded that the edges of each bed corresponded originally to the margin of some sea, and its lower surface to the sea-floor. In a series of beds all except the lowest were originally contained between two planes parallel to the horizon. In these statements there is contained implicitly the modern doctrine of super-position, *i.e.* that the order in which beds succeed one another vertically in space is the order in which they have been deposited, or the order of their succession in time. This is explicitly stated by Steno in another paragraph "at what time there was formed any Bed, the matter incumbent on it was all fluid, and by consequence, when the lowest Bed was laid, none of the upper Beds was extant".

Steno however pursued his inquiry far beyond this stage; he proceeded to point out that some strata occupy an inclined or even vertical position, and be rightly inferred that these must have been tilted out of the horizontal by some natural disturbance subsequent to their formation.

The crowning triumph of Steno's achievement lay in the application which he made of these conclusions to the explanation of the structure of Tuscany.

The structure of Tuscany as Steno conceived it is shown in the following diagram, copied from his work: *De Solido intra Solidum Naturaliter Contento.* The mountains are for the most part composed of horizontal beds, C, F, but dislocated and sloping towards the low ground, on their flanks. On these fundamental rocks others of a later date, C, A, B, rest discordantly; and thus it appears that Steno was familiar with the important phenomenon now termed an unconformity. Now since the strata were originally deposited as continuous horizontal layers, it follows that the rocks C, A, B at one time extended in an unbroken sheet between the flanks of the surrounding mountains, as shown in Fig 2. Further since the bed B, A, C could not have

been self-supporting, but must have rested on older sediments the gap below it must have been filled up with a succession of horizontal deposits as shown in Fig. 3. This state of things again was not original, there was a time when these sediments did not exist, and the Tuscan district



appeared as in Fig. 4. Once more, the argument which applied to the latest rocks, holds equally for the older ones G, F, so that we must restore these, first as represented in Fig. 5, and next as in Fig. 6. Beyond the last stage, when Tuscany lay inchoate, a mass of sediments beneath a primæval sea, we can only take one step farther back into the

abyss of time, and this brings us to a period, antecedent to the deposition of any sediment whatever, and to a state of Tuscany which Steno does not venture to represent by a diagram.

Steno did not regard the explanation, just set forth, as restricted in its application to Tuscany; but distinctly affirmed, from his knowledge of other countries, that similar reasoning would hold for every part of the world, where stratified rocks occur.

The first to logically demonstrate the true nature of fossils; the first to recognise the orderly sequence of deposits; the first to perceive their occasional interruption by those discordances, which we now term unconformities; the first to employ geological structure to arrive at geological history and to inform us of the revolutions through which our globe has passed; said we not truly that this man was the father and founder of our science?

In attempting to carry his explanations farther and to push them deeper, Steno was less happy: his brave imagination, successful so far, was now to suffer from the restraints imposed upon it by a belief in the brevity of the world's existence. The stage which immediately preceded that represented by Steno's sixth diagram stood before his mind as the beginning of created times, and all the subsequent events of the earth's history had to be compressed into a poor 6000 years! Without a more generous allowance of time no genius, however gifted, could hope to trace the slowly pacing processes of nature; and the fame of Steno would undoubtedly have burned brighter had he desisted from the attempt and refrained from an endeavour to reconcile the irreconcilable.

Contemporary with Steno was the celebrated Dr. Plot of this University, whose great work on *The Natural History of Oxfordshire* appeared in 1677, eight years after the publication of Steno's *De Solido*, etc. This was 220 years ago, in the middle of the reign of Charles II., about fifty years after the Fellows of Wadham College and their friends met together to bring into existence the Royal Society, for which the good king did so much by judi-

ciously letting it alone; modern chemistry had but just sprung into being under the influence of Robert Boyle, and Newton's *Principia* had not yet enlightened the world.

The History of Oxfordshire is a very interesting work, full of queer old-world information, quaint digressions and pleasant meanderings, winding gently on each side of the main current of discourse It contains a map of Oxfordshire curiously bordered round with the arms of the resident nobility and gentry, and each of its excellent plates of illustrations bears a dedication in the corner to some noble patron, with the emblazonment of his arms.

Dr. Plot was evidently acquainted with the views of Steno, but he did not share them; on the contrary, he met them with a strenuous opposition, in which he displayed great resources of learning and dialectic skill. A whole chapter of the *History* is devoted to the description of the fossils or, as they are there termed, the "Formed Stones" of the county. It is illustrated by engravings which, for truthfulness, leave little to be desired, so that, even at this distance of time, there is little difficulty in recognising and identifying the different genera or even species that they represent.

The stones to which Dr. Plot first directs attention are those that stand in some connection with the heavenly bodies, such as the Sun-stone, of which, however, there are none in Oxfordshire; the Moon-stone or Selenites; the Asteriæ or Star-stones, which are evidently the joints of the stem of a fossil, now called *Extracrinus briareus;* the Astroites, bodies of irregular form, but adorned with constellations of stars; these are clearly corals, such as are now named Isastrea and Thamnastrea; and finally such stones as are supposed "by the *vulgar* at least" to be "generated in the *Clouds* and discharged thence in the times of *Thunder* and *violen Showers*"; such are Belemnites, and Brontiæ and Ombriæ: in the two latter we now recognise several kinds of Sea-urchins. The author then passes to "The *Stones* that concern the *Watery Kingdom*"; some of these are spars or minerals; others are true fossils such as a "Strombites," which is compared to the living "Concha Tridacna," so

called, the author quaintly, though erroneously, observes, "because they made three Mouthfuls apiece," and he ingenuously adds that were "the Strombites not a Stone I must pronounce it the Same," *i.e.*, as the living Tridacna. A "Conchites" of the kind we now term "Rhynchonella" is introduced to us, and in a digression we are informed that specimens of this "made red hot and put into drink are accounted a present Remedy for a Stitch".

There is also a Pecten, which the author compares with the *Pecten asper* of Aldrovandus, and an Oyster (evidently *Ostrea dilatata*) of which it is remarked "I could easily have assented that these . . . might once indeed have been Shell-fish, but that (just as with the Escallops) we only find the *protuberant parts* of the Shells [convex valves] and never any of the *flat ones*". We then come to a fossil, now known as Cidaris, which is spoken of as "a curiously embroidered Stone, much resembling the petrified *Riccio Marino*, or Sea-Urchin of *Imperatus*," also known to old authors as *Mamelles de Saint Paul* and as *Ova Anguina* "because from the *Basis* there issue as it were five *Tails* of *Serpents*, waved and attenuated towards the Upper part of the *Stones*". These "Old Authors" regarded it as "engendered from the *Salivation* and *Slime* of *Snakes*, and cast into the Air by the Force of their *Sibilations:* where if taken, has Effects as wonderful as its Generation, and therefore of great Esteem amongst the *French Druids*". "But," concludes our excellent author, "I care not to spend my time in *Romance* and therefore proceed," and so passes to the Cornua Ammonis or Ophiomorphites, our Ammonites. Some of these are found "about Adderbury, about two Miles from *Banbury*, but . . . that the Town has not its Name from these Stones (as Mr. Ray thinks) I dare confidently avouch, *Adderbury* being only the vulgar Name: for in the *Court-Rolls* of *New College* . . . it is written *Eabberbury*, perhaps from St. *Ebba*, the tutelar Saint of the *Church*".

The "Formed Stones" having been sufficiently described, Dr. Plot proceeds to a discussion of "The great *Question* now so much controverted in the World: Whether

the Stones we now find in the form of *Shell-fish* be *Lapides sui generis*, naturally produced by some extraordinary *plastic virtue*, latent in the Earth or Quarries where they are found? Or whether they rather owe their Form and Figuration to the *Shells* of the *Fishes* they represent, brought to the *places* where they are now found by a *Deluge*, *Earthquake* or some other such means, and then being filled with *Mud*, *Clay*, and petrifying *Juices*, have in tract of time been turned into *Stones*, as we now find them, still retaining the same Shape in the whole, with the same *Lineations*, *Sutures*, *Eminences*, *Cavities*, *Orifices*, *Points* that they had whilst they were *Shells?*" "In the handling thereof" the author modestly disclaims any intention of arriving at a "peremptory *Decision*" and invites merely to "a friendly *Debate*". Let no one be beguiled, however, by these fair words, which are but the bow of a champion on entering the arena. It is true the debate proceeds smoothly enough, but it is also conducted according to all the rules of fence and with the art of a master. Steno's weakest point was the deluge, which he had to bring over Tuscany in his final explanation of geologic changes. Plot, with unerring instinct, makes straight for this. He considers first the difficult question of the means by which these fossils, if they were originally parts of living animals, could have been transported from the sea to the interior of the country: deluges had been suggested, but with deluges, whether Noachian, Ogygian, Deucalonian or purely local or national floods, Plot will have no dealings, alleging very sufficient reasons for regarding all deluges as unfitted, by their very nature, for the effects required; while as to earthquakes, he remarks, that to suppose "the Mountains (where such Stones as most resemble them [shell-fish] are now found) were heretofore low places and since raised by *Earthquakes:* [is] a thing by no means to be believed of our *Northern* Parts, where the *Earthquakes* we have at any time are so inconsiderable that they scarce sometimes are perceived, much less affrighten us; unless we shall groundlessly grant, that in the infancy of the *World*, the *Earth* suffered more concussions, and consequently more mutations in its *Super-*

ficies, than it has done ever since the Recordsof Time":—an argument that cannot but appeal to every orthodox Uniformitarian.

Having thus shown an antecedent improbability against the view that fossils are the remains of organisms, he proceeds to attack the enemy in his stronghold. It is affirmed, Dr. Plot remarks, that "these *Formed Stones* are many of them in all respects, like the living *Shell-fish:* thus says Boccone, the *Herrison's Spatagi* of *Stone*, the *Cornua Ammonis* or *Nautili Lapides* have the very Marks, Characters, Eminences, Cavities and all other parts alike, with the true living *Nautili*, and *Herrison's Spatagi*, . . . which proves, says he, *the Body changed to have been the very same thing with that which is living.* But I must tell him, it does but very weakly, all Arguments drawn *a similitudine*, being the most inefficacious of all others, such rather illustrating than proving, rather perswading than compelling an *Adversary's* Assent. For how many hundred things are there in the *World*, that have some Resemblance of one Another, which no Body will offer to think were ever the *same*, and particularly among some other *Formed Stones* hereafter to be mentioned. Such are the *Stones Otites*, or *Auriculares*, several sorts of *Cardites*, which though they as exactly resemble those parts of *Men*, from whence they have their *Names* as any *Conchites* or *Echinites* do those *Shell-fish:* yet no *Man* that I ever heard of, so much as dreamed that these were ever the real parts of *Men*, in process of time thus turned into *Stone*. As well might we say that our *Kettering Stone* in *Northamptonshire*, here in *England*, was once nothing else but the spawn of Lobsters: than which that I know of, there is nothing more like." If I might express an opinion, I should say, that as it stands, this is a very excellent argument: but it would have been open to an opponent to reply that it is vitiated by one serious mis-statement: the simulacra of human organs which are referred to as Otites and Cardites do *not* as exactly resemble those organs, as the Conchites and Echinites do those shell-fish: in the latter case the resemblances are far more numerous and precise, and are if

anything, more marked in the internal structure than the external form; so, that even when agreement in form fails, similarity in structure which is all important remains. This cannot be said of the Otites and Cardites.

The argument is then pushed a step farther by pointing out that the resemblance between organism and formed stones is frequently not so close as is asserted: thus the Cornua Ammonis had been likened to the shells of Nautilus from which they certainly differ in important particulars; but in addition to these minor points of difference Dr. Plot triumphantly cites instances in which they had been observed with "heads". "'Vidimus enim lapidem hinc delatum surpentis in spiram revoluti effigie, cujus caput in circumferentia prominuit, extrema cauda centrum occupante,' are the very words of Mr. *Cambden*".

Although this instance is an unfortunate one, since the "heads" were no doubt the work of some practical joker or fraudulent fossil finder, yet the argument will not be greatly affected, since it is admitted that the fossil forms often differ markedly from their living representatives. The explanation of these differences had been arrived at long before by Palissy, and is suggested by a writer, bearing the illustrious name of Lister, who says truly that these fossils must be either "terrigenous, or if otherwise, the animals they so exactly represent have become extinct". Referring to this Dr. Plot remarks: "If it be said, that possibly these Species may be now lost, I shall leave it to the *Reader* to judge, whether it be likely that *Providence*, which took so much care to secure the works of the *Creation* in *Noah's Flood*, should either then, or since, have been so unmindful of some *Shell-fish* (and of no other *Animals*) as to suffer any one *Species* to be lost".

Since this was written we have learnt to accept the doctrine of the extinction of species; it has become a common-place amongst the educated, and has passed into the language of our poets:—

> So careful of the type she seems! But no!
> From scarped cliff and quarried stone
> She cries a thousand types are gone!
> I care for nothing; all shall go.

After his exposition of the weak points in Steno's argument Dr. Plot proceeds to consider the alternative view, that the formed stones are the result of some "plastic force"; and as later writers have made merry over this expression, it will be merest justice to explain what Dr. Plot meant by it. Whatever the plastic force of Theophrastus may have been, that of Dr. Plot was certainly what we now recognise as crystallisation. His words are: "There is no other Principle that we yet know of naturally shooting into *Figures*, each peculiar to their own kind, but *Salts*, thus *Nitre* always shoots into *Pyramids*, *salt Marine* into *Cubes*, *Alum* into *Octo*, and *Sal Ammoniac* into *Hexahedrums*, and other mixt *Salts* into as mixt *Figures*.

"Of these spontaneous inclinations of *Salts* each peculiar to its *Kind*, we have further evidence in the *Chymical* Anatomy of *Animals*, particularly in the *Volatile Salt* of *Harts-horn*, which in the Beginning of its Ascent is always seen branched in the Head of the *Cucurbit*, like the Natural *Horn*. And we are told by the very ingenious and learned *Sidleyian Professor* here in *Oxon*, That the *Salt* of *Vipers* ascends in like manner, and shoots into *Shapes* somewhat like those of *Animals*, placed orderly in the *Glass*. Thus in *Congelations*, which are all wrought by adventitious *Salts*, we frequently find curious *Ramifications*, as on Glass-windows in Winter, and the figured Flakes of *Snow*, of which Mr. *Hook* observed above an Hundred several sorts, yet all of them branched as we paint *Stars*, with six principal *Radii* of equal Length, Shape and Make, issuing from a *Center*, where they all joined in *Angles* of 60 *Degrees*.

"What *Salt* it should be that gives this *Figure*, though it be hard to determine, yet certainly it must not be a much different *one* from *that* which gives Form to our *Astroites* and *Asteriæ*, whereof, though the latter have but five *Points*, and therefore making *Angles* where they are joined at the *Center* of 72 *Degrees*: yet the *Astroites* both in *mezzo Rilievo* and *Intagli* have many more.

"Perhaps there may be something of an *Antimonial Salt* that may determine Bodies to this *Starry* Figure, as no Question it does in the *Regulus*, and the *Caput mortuum* of

the *Cinnabar* of *Antimony*. To such a *Salt* may also be referred our *Brontiæ* or *Ombriæ*, and all the *Echinites*, *some* whereof are plainly, *all* in some measure *stellated* at the Top.

"The *Belemnites*, which are all *striated* from a *Center*, yet in the whole affect a *Pyramidal* Form, seem to have somewhat also of an *Antimonial*, but a more prevalent Quantity of a *Nitrous Salt*.

"The *Conchites*, *Pectinites* and *Ostracites*, whether transversely *striated*, or from the *Commissures* to the *Rim*, seem to owe their origin to *Urinous Salts*, which start likewise from a *Center* (as suppose from the Hinges of these *Stones*), but generally are most extended to one side, as may be seen in the branched Figure formed on the Surface of *Urine* by freezing, as in Mr. *Hook's Micrography*, whose *Striæ* not obtaining much above the *Quadrant* of a *Circle*, whatever other Differences they may be, in this respect at least is agreeable to our *Stones*.

"To which add the *Ophiomorphit's* or *Cornua Ammonis*, most probably formed either by the *Salts* shooting different ways, which by thwarting one another make a *Helical* Figure, just as two opposite Winds or Waters make a *Turbo*, or else by some simple, yet unknown *Salt*, that affects such a Figure: perhaps the Stems and Branchings bended in a most excellent and regular Order, like the Ribs of some of our *Ophiomorphit's*, observed by Mr. *Hook* in *Regulus Martis Stellaris*, might not a little conduce to the clearing of this Matter."

The difficulties which beset this explanation are duly considered and disposed of; the last of them is so quaintly treated that I cannot forego the pleasure of quoting it. "That it seems quite contrary to the infinite *Prudence* of *Nature*, which is observable in all its Works and Productions, to design every *thing* to a determinate *end*, and for the attaining of that *End*, makes use of such ways as are (as far as the Knowledge of *Man* has yet been able to reach) altogether consonant and agreeable to *Man's* Reason, and of no way or means that doth contradict, or is contrary to *Human Ratiocination:* Whence it has been a general Ob-

servation and *Maxim*, that *Nature doth nothing in vain.* It seems, I say, contrary to that great Wisdom of *Nature*, that these prettily shaped Bodies should have all those curious Figures and Contrivances (which many of them are adorned and contrived with) generated or wrought by a *plastick Virtue*, for no other higher End than only to exhibit a *Form*. To which I answer, that Nature herein acts neither contrary to her own *Prudence, human Ratiocination* or in *vain*, it being the Wisdom and Goodness of the *Supreme Nature* (by the Schoolmen called *Naturans*) that governs and directs the *Natura naturata* here below, to beautify the World with these Varieties, which I take to be the End of such Productions as of most *Flowers*, such as *Tulips*, *Anemonies*, etc. of which we know as little Use as of *Formed Stones*. Nay, perhaps there may proportionately Number for Number be as many of *them* of *Medicinal* or other *Use*, such as *Selenites*, *Belemnites*, *Conchites*, *Lapis Judiacus*, etc., as there are of Plants: so that unless we may say also (which I guess no Body will) that these are produced contrary to the great Wisdom of Nature, we must not of *Stones*."

There were however certain fossils, known to Dr. Plot, the organic nature of which he was ready to admit, for they possessed not only the outward form of bones, but exhibited, though turned to stone, a characteristic bony structure. One of these was a thigh bone, of what nature it would from the description be difficult to say; it was found in the parish of Cornwell and is thus considered by the author: "It remains", he says, "that it must have belonged to some greater *Animal* than either an *Ox* or *Horse:* and if so (say almost all other *Authors* in like *Case*) in all probability it must have been the *Bone* of some *Elephant*, brought hither during the Government of the *Romans* in *Britain:* but this Opinion too lies under so great Difficulties, that it can hardly be admitted; which are briefly these:—None of the *Roman Authors*, who elsewhere are large enough in describing the *Elephant's* Behaviour in *Fight*, and how terrible they were to some of the *Trans-Alpine* Nations, mention any such Matter in any of their *Expeditions* into

Britain. . . . One there was 'tis true sent hither as a Present by St. *Lewis* IX., King of France to King *Henry* III. *Anno* 1255, which, says *Matthew Paris*, was the first seen this side of the *Alps*, and perhaps there may have been two or three brought for Show hither since: but whether it be likely any of these should have been buried at *Cornwell* let the *Reader* judge."

A point is made of the fact that tusks had not been found associated with the supposed elephant's bones, a piece of negative evidence, the worthlessness of which is shown by the subsequent discovery of such tusks.

Instances are then given of the exhumation of similar bones from churchyards. "Since the great Conflagration of *London*, *Anno* 1666, upon the pulling down of St. *Mary Wool-Church*, and making the site of it into a *Market-place* there was found a *Thigh-bone* (supposed to be of a *Woman*) which was to be seen at the *King's Head Tavern* at *Greenwich* in *Kent*, much bigger and longer than ours of *Stone* could in proportion be, had it been entire." Two other similar bones were dug up in the parish churchyard of Merton Valence. "Now," exclaims our author in triumph, "how *Elephants* should come to be buried in *Churches*, is a Question not easily answered, except we will run to so groundless a shift, as to say, that possibly the *Elephants* might be there buried before *Christianity* florished in *Britain* and that these Churches were afterwards casually built over them."

It is possible that the thigh bone in which Dr. Plot was so much interested was not that of an elephant, for he says: "But what is *instar omnium* in this difficult point, there happily came to *Oxford* while I was writing of this, a living *Elephant* to be shewn publickly at the Act, *An.* 1676, with whose *Bones* and *Teeth*.[1] I compared ours: and found those of the *Elephant* not only a different Shape, but also incomparably bigger than *ours*, though the Beast were very young and not half grown."

The conclusion follows that the thigh bone is human

[1] The teeth were evidently those of a ruminant.

and belonged to a giant: many marvellous accounts of giants are then cited, some clearly fabulous, and not carrying conviction to the author himself, who proceeds as follows: "But to come closer to the Business, and more determinate Statures, the same *Pliny* tells us of two others living in the time of *Augustus*, nicknamed *Pusio* and *Secundilla*, whose Bodies were preserved for a Wonder in the *Sallustian* Gardens, that were ten Foot high; and that in his time, there was one *Gabbara*, brought out of *Arabia* in the days of Prince *Claudius* the Emperor, exactly of the height of *Goliath*, *viz.*, nine Foot nine Inches high, which being a Size very proportionable to our Bone found at *Cornwell*, I am rather inclined to believe, that *Claudius* brought this *Gabbara* into *Britain* with him, who might possibly dye and lay his Bones here, than that ever they belonged to any *Elephant*: except we shall rather say that here also *Corinaeus*, Cousin to *Brute*, might kill one of *Gogmagog's* Race, and that from him the Place doth take its Name, as well as the County of *Cornwall*."

In reflecting on these views of Dr. Plot, no one will fail to recognise his perfect good faith; he does not appear to have been greatly influenced by theological prepossessions and is honestly anxious to arrive at the truth.

What then, it may be asked, was it that led him, and and many great naturalists with him, to conclusions so opposed to those of Steno, so opposed as we now know to the truth itself?

There were evidently several reasons. In the first place, Plot clearly perceived that the admission of the organic nature of fossils brought with it a whole train of perplexing consequences. To avoid these, he preferred an appeal to crystallisation, as raising ultimately fewer difficulties. Other branches of Science were not at that time sufficiently advanced to show the baselessness of this explanation, the subject was a difficult one, many additional observations were needed, and the mind of Plot was eminently critical. When we survey the vast superstructure that modern Science has raised on the foundation prepared by Steno, *i.e.* on the organic nature of fossils, and the super-

position of stratified rocks; none but an impatient mind will feel other than grateful to Plot for having subjected these fundamental principles to the severest examination, for having spared no argument which could possibly be brought against them. Next to suggestive generalisation, Science stands in need of honest criticism. Geology required a prophet, and she found him in Steno, but she also required a critic, and in Plot she met with one of the most penetrating intellect and uncompromising spirit.

Both sides of this momentous controversy had now been fairly presented to the world, and it was left for the faithful labours of the next century to arrive at a decision. The principles of Steno eventually prevailed, and Geology entered upon a new phase of her existence; she commenced to free herself from the trammels of time!

Hutton in his great work *The Theory of the Earth*, published in 1788, attacked the subject on that side where Steno had most conspicuously failed. His method was the same as Steno's; just as Steno had explained fossil remains by comparison with living animals, so Hutton explained the past history of the earth by comparison with the present. If this county of Oxfordshire was, together with all England, at one time submerged fathoms deep beneath the sea, it was owing to a slow movement of subsidence such as Hutton considered was affecting islands and continents at the present day. Would we account for the marine sediments of which our land consists, we have but to turn our eyes to the rivers constantly bearing their burden of mud into the sea, where it is spread abroad in strata precisely similar to those of the remote past. As strata are formed now, so have they always been formed; as lands rise and fall now, and seas pass imperceptibly into continents, and continents into seas, so they have always done in the past. But how slowly all these changes proceed! A human lifetime is not long enough to witness any appreciable effect. In this period the thickness of stratified material laid down by a river in the sea would not amount to more than a few inches, and the rocks of the earth's crust are many thousands

of feet in thickness. Well might the stoutest hearted be appalled before the vast æons of time which Hutton conjured up before the imagination! Not so Hutton; having broken the narrow bonds of time previously imposed upon our science, he revelled in excess of freedom, and declared that the duration of our planet was practically infinite!

How was this new teaching received in Oxford?

The title of a work by Dr. Kidd, who was Professor of Chemistry in 1803, will inform us. It runs as follows: *A Geological Essay on the Imperfect Evidence in Support of a Theory of the Earth*, 1815; and the book itself concludes with the assertion that "ancient strata cannot be explained by existing causes: that the Science of Geology is so completely in its infancy as to render hopeless any attempt at a successful generalisation". In this opinion I believe Dr. Kidd to have been in sound agreement with the teaching of the leading geologists of the day: his work is by no means aggressive, and might rather be regarded as a text-book than a polemic: as a text-book it was well "up to date," and we are impressed on reading it with the great advance which had been made since the time of Plot.

Here must be mentioned the name of William Smith, a native of Oxfordshire, though not a member of this University. His various works were published between the years 1790 and 1815, and to him we owe a discovery that excited no controversy, but which nevertheless was the greatest in Geology made since the time of Steno. It was that different strata contain different kinds of fossils, which are peculiar to them, and thus serve as marks by which the strata can be identified; so that with the aid of fossils it has become possible to trace the same group of strata across the length of the British Isles, nay, even throughout the whole of Europe and into distant parts of the world. This discovery made Geology for the first time possible as an exact science.

The successor to Dr. Kidd was a geologist of the very highest rank, one eminently great amongst a crowd of great contemporaries. I allude to the famous Dr. Buckland.

The period of Buckland has been styled, and justly

styled, the Golden Age of Geology. Sedgwick was his contemporary in Cambridge, Phillips, afterwards to succeed him, was his contemporary in Dublin, Murchison learnt his first lesson in the field from him, Lyell was his pupil, Agassiz a coadjutor, and Conybeare his nearest friend.

When Buckland was appointed Reader in Geology, the foundations of the science were already laid, but great problems remained for solution. The question of the deluge and how far its effects could be recognised in the structure of the earth's crust was still one of these.

Buckland appeared at first as a champion of the deluge: thus in his inaugural lecture delivered before this University in 1819, he expressed himself in the following words: "The grand fact of an universal deluge at no very remote period is proved on grounds so decisive and incontrovertible, that had we never heard of such an event from Scripture or any other Authority, Geology of itself must have called in the assistance of some such catastrophe to explain the phenomena of diluvial action".

Subsequently pursuing his researches into this question he seems to have ransacked the whole world for evidence and found everywhere confirmatory proofs. The ancient gravels of Wytham were, he considered, swept from Warwickshire and counties still farther north, in the rush of the great flood; stones from Norway were carried to the east coast of England, and the mass of pebbles and other *débris*, which were driven along with it, scoured the face of the country and thus produced the polishing and striation so frequently visible on the surface of the harder rocks of our islands; the bones of mammoths and other mighty monsters of prediluvial times, that lie buried in caves and elsewhere, were eloquent in their testimony to the destruction which it wrought on the living world. That the loftiest mountains were submerged by it was proved by the discovery of the bones of horses and deer at an altitude of 16,000 feet above the sea on the slopes of the Himalayas. They are brought down by the avalanches of those mountains, and are said by the natives to have fallen from the clouds and to be the bones of genii. The *Reliquiæ Diluvianæ*, in which these

views appear, was published in 1823, and by its skill, learning, and eloquence at once attracted universal admiration.

But Buckland, though he appears in this work as an advocate, was by no means merely an advocate, his was a mind too highly endowed to rest satisfied with any but the most convincing proof, and as time elapsed, and he extended his researches, the evidence, which he so industriously accumulated, so far from strengthening his position, began to gradually undermine it, and already in 1837, when he published his great work on *Geology and Mineralogy Considered with Reference to Natural Theology*, we find him wavering. While still asserting the occurrence of a diluvial catastrophe, he was prepared to abandon the view, which would connect this with the Noachian deluge. "It has been justly argued," he writes, "that as the rise and fall of the waters of the Mosaic deluge are described to have been gradual and of short duration, they would have produced comparatively little change on the surface of the country they overflowed. . . . This important point, however, cannot be considered as completely settled till more detailed investigations of the newest members of the Pliocene and of the diluvial and alluvial formations shall have taken place."

Scarcely a year had elapsed after this opinion had been expressed when investigations were commenced, which were to cast a flood of light on the question from an unexpected quarter. Agassiz in 1838 was already engaged in those observations on the glaciers of Switzerland, and had commenced that series of brilliant discoveries which eventually culminated in a clear and reasonable explanation of the so-called diluvial phenomena. Buckland's study of these phenomena in Europe and the British Isles had rendered him the first authority of the time on this subject, though the cause of these phenomena, as we now know, was really not diluvial as he then imagined, but glacial. Buckland therefore was perfectly familiar with all the signs now recognised as characteristic of ice-action, no man more so: consequently when Agassiz took him over the glaciers of Switzerland, and showed him these agents actually at work,

smoothing, polishing and striating the surface over which they flowed, the evidence was presented to a mind already prepared to appreciate it, and a few days' personal investigation sufficed to convince Buckland of the truth of Agassiz' opinions. This was not a case of a mere tyro, introduced to the subject for the first time, but of a skilled and trained observer, familiar by experience with results, the cause of which he had for long been trying vainly to discover. Here however it may be better to allow Dr. Buckland to tell the story in his own words: among his MSS. I have found a rough draft of a letter, evidently written to Agassiz, in which he says: "In October, 1838, I for the first time attended to the effects of Glaciers, which you pointed out to me in the phenomena of polished and striated and furrowed Surfaces in the S. E. slope of the Jura, near Neuchatel, the origin of which, as well as the Transport of the Boulders of Granite, etc., from the Alps to the Jura, you referred to the Agency of ancient Glaciers. Not then satisfied with your Explanation, I proceeded to devote some days to the examination of actual Glaciers, and the result was my conversion from a sturdy Opponent to the adoption of your Theory as far as relates to Switzerland by the strict accordance which I found between many residuary Phenomena of existing Glaciers in the high Alps, and similar residuary Phenomena that are equally apparent on the S.E. slope of the Jura, fronting the Alps" [I mentioned to you] "that Sir J. Hall had in 1812 described similar polished surfaces, grooves and furrows in the vicinity of Edinburgh, and that other observers had occasionally noticed them on the surfaces of hard Rocks, which have been protected from the weather in many parts of Scotland and England. I also proposed to conduct you to some of the most remarkable of these spots on your visit to England in 1840, which you have just accomplished."

How justly Buckland estimated the importance of this great discovery is shown by notes in his handwriting, that were probably used to assist him in one of those remarkable speeches the echoes of which still reverberate in our time. In one he says: "For some time to come the Glacial

Theory must occupy a prominent place in Geological Investigation. The Subject appears to me the most important that has been put forth since the propounding of the Huttonian Theory: and the surface of the whole Globe must be examined afresh with the View of ascertaining how far the Effect of Glaciers and Ice-bergs and of Floods produced by melting ice and snow can be found and identified with the actual effects of Ice and Snow in our present Polar and Alpine Regions." This prediction has been verified; even now, fifty years after these words were uttered, observers are still engaged in the investigation of glacial phenomena; the subject still occupies as prominent a place as it did then, and promises to do so for an indefinite time yet to come. In another note Buckland comments on "The vast field of new Enquiry which the introduction of the Glacial period between our Epoch and the Newest Tertiary opens to the Geological Enquiry. The fact of the Greater Part of Europe and North America having for many years been sealed up under a cover of frozen Snow converted to the state of Glaciers is certain. . . . Thus the flood that caused the Diluvium which in my *Bridgewater Treatise* I have put back to the latest of the many Geological Deluges, was probably due to the melting of the Ice. The details of this Ice flood will fill a Volume and will constitute Vol. II. of my *Reliquiæ Diluvianæ and Glaciales*, which for fifteen years has been retarded for lack of the grand key which Agassiz has supplied in his *Etudes des Glaciers*."

Thus Buckland courageously recanted his earlier opinions on the deluge; and of the Noachian deluge as a geologic agent from this time forth we hear no more. This was one of the greatest benefits that Buckland conferred on Geology; commencing as the most powerful champion that had ever appeared on the side of the Noachian deluge, he step by step and by slow degrees was led to reconsider his position, till finally he abandoned it altogether, and left Geology freed from a mistaken doctrine to pursue her peaceful path, disturbed by no difficulties, beyond those naturally inherent in the sub-

ject. For this alone we owe him a debt of deep gratitude, but our obligations do not cease here, he introduced to the eyes of an astonished world a strange procession of extinct forms of life, with whom he seemed on most familiar terms; parodising a line from Juvenal he exclaimed, "Quicquid agunt 'Sauri,' votum timor ira voluptas gaudia discursus nostri est farrago Libelli" and then proceeded to describe "the details of their political and domestic economy".

In his *Bridgewater Treatise* we may still read with interest and profit his vivid account of the Icthyosaurs, Plesiosaurs, Megalosaurus, and Pterodactyles, and other monsters of the mighty past. This work was written sixty years ago, and yet such was the sagacity of the author in selecting only those facts which were well ascertained and sure, that it may be put into the hands of the youngest and most innocent of geologists, without fear of infecting his mind with forgotten error.

Nor must we here overlook his success in informing and convincing the world of science of the fact that in times immediately antecedent to the glacial period, our country and Europe generally was the home of a vast number of animals no longer found in it, some of them altogether extinct, such as the mammoth, rhinoceros, hippopotamus, cave hyæna, bears and tigers, and the arctic glutton.

Of Buckland's pupils, the greatest was Lyell, whose fame indeed overshadows his master's. Hutton had propounded the theory of the efficacy of modern causes, Playfair had illustrated it, and Lyell spent his life in defending and elaborating it. In consequence of his success in this work, it is far more often associated with Lyell's name, who was its foster parent, than with Hutton's who was its true originator.

We have already alluded to this doctrine. It is that the only method by which an explanation of the past can be obtained is by a careful study of the processes of the present, or, as it is often epigrammatically expressed, "Geology finds in the processes of the present a key to unlock the past". It must be confessed at the outset that it is difficult to see where else she would find it; not in the

future, of which we know nothing, and scarcely in the past itself, whose problems are to be explained. Epigrams are frequently only a neat way of obscuring a truth, in this case the fact would appear to be that most of the older observers, when they encountered interruptions in the succession of strata, were accustomed to explain them by interruptions in the orderly progress of nature, by sudden and violent changes, which were spoken of as catastrophes. Hutton and Lyell were able to offer explanations of these without the invocation of catastrophes. They certainly introduced the scientific method into cases which had previously been treated by a too free use of the imagination, and thus their doctrine, the doctrine of uniformity, has dominated geologic thought for the past sixty years, and will probably continue to do so for many years to come. The teachings of Hutton and Lyell have, however, another side; they assume not only that existing causes have acted in the past as in the present, but at the same uniform rate: this was the natural result of a reaction, which followed when Geology was loosened from her ancient bondage to time, and under the influence of which geologists came to regard the periods at their disposal as practically infinite. The mathematician often employs in his calculations the device of assuming some very large quantity to be infinite, and in this way obtains approximate results sufficiently close for working purposes. This is precisely what Hutton and Lyell did in their explanations. But when a limit becomes assignable to this quantity, the mathematician will abandon the fiction of infinitude and introduce the ascertained or estimated value into his equations with a view to arriving at greater exactitude. Of late years it has been asserted by a very high authority—Lord Kelvin—that a limit can be assigned to geological time; once more Geology is put under bondage, not however, as in her youth, tethered to a mere 6000 years, but free to roam through the ample magnitude of 30,000,000! It is at present impossible to say how near the truth Lord Kelvin's estimate may be; it is founded on data which may be inexact and on assumptions which may be illegitimate, but that it is approximately correct the preponderance of evidence seems to show.

That the strict uniformitarian view is as false in philosophy as it is unfounded on fact is an opinion which was shared by the late Professor Prestwich, and which is held by many of the Geologists of to-day, as it was by the great masters of old. It is tantamount to asserting that the progress of events on the earth can be represented by a curve which is a straight line. We should think strangely of the physicist who from the behaviour of a fluid through the range of ordinary atmospheric temperature should proceed to deduce from it a rate of expansion up to the temperature of the sun in one direction and down to absolute zero in the other; and yet this would scarcely show greater wisdom than the procedure of the geologist who from a knowledge of the earth's history during the past few thousand years should endeavour to deduce from it the rate of events during 30,000,000 of years in the past!

Dr. Buckland was succeeded by Professor Phillips, a man of most varied genius, a classical scholar, an expert mathematician, an omnivorous reader, facile both with pencil and pen, interested in all science and a master in his own. He taught in this University for more than twenty years, and during that period he enriched our science by numerous contributions of the highest value. A smooth and easy progress marked the course of Geology, and knowledge steadily enlarged its bounds. The great Cetiosaurus, one of the greatest of the old world monsters, larger even than the great Iguanodon which is now represented in our museum, we owe to him. Towards the end of his career, Geology like all other science was confronted by the reappearance of an old and discredited doctrine, but now presented afresh with new and startling vigour, it was the doctrine of Evolution as expounded in the famous *Origin of Species by Natural Selection.* Once more an Oxford professor was called upon to pronounce judgment on one of those momentous questions which arise from time to time to disturb the steady current of established thought.

Darwin's present of a copy of his book was accompanied by the following letter:—

My Dear Phillips,

I have directed Murray to send you a copy of my book on the *Origin of Species*, which as yet is only an abstract. I fear that you will be inclined to fulminate awful anathemas against it. I assure you that it is the result of far more labour than is apparent in its present highly condensed state. If you have time to read it, let me beg you to read it all straight through, as otherwise it will be unintelligible. Try not to condemn it utterly till you have finished it and reflected on the recapitulation. Not that I am so foolish as to expect to convert any one, who has long viewed the subject from an opposite point of view. I remember too well how many long years my own conversion took. The utmost which I hope is that you may see that more can be said on the side of mutability of specific forms than is at first sight apparent. If indeed your own observations have made you at all sceptical on the subject, then my Book may produce some effect. . . .

Yours very sincerely,

Charles Darwin.

Phillips had for a long time previously given careful attention to the "Succession of Life on the Earth," and had chosen this subject for the Read lecture, which he delivered before the University of Cambridge, shortly before the appearance of the *Origin of Species*.

His pronouncement on Darwin's work was adverse. "Dead against," as Darwin wrote. His opinion as expressed in a letter to Darwin, of which he did not preserve a copy, called forth the following reply :—

Ilkley Wells House,
Otley, Yorkshire, 26*th November* [1859].

My Dear Phillips,

Thank you for your note. Permit me to say one word about my book. Though many facts in Palæontology may appear, or be really, opposed to my notions, and though my explanations may be quite fallacious, I earnestly beg you to consider whether a theory wholly false would explain, as it seems to me to explain, several classes of facts—as affinity of inhabitants of islands to nearest continent; the nature of the inhabitants of oceanic islands; the affinities and classification of organic beings and their arrangement in groups; the strange fact of a member of one group being adapted to the habits of another group; the facts of morphology or homology; embryology and rudimentary organs. If you think the theory of Natural Selection does not to a large extent explain these classes of facts, I have not a word to say. Pray forgive me saying a word in favour of my own offspring to one whom I consider an important judge.

Yours very sincerely,

C. Darwin.

That Phillips betrayed no bigoted opposition to the doctrine of Evolution is shown by several attempts which he himself subsequently made to construct a phylogeny of different groups of animals from a knowledge of their fossil remains, but while he succeeded in tracing several interesting lines of descent among species, he confessed himself unable to bring the more widely separated groups or genera into an ancestral connection. Since these early attempts of Phillips, we have learned not only to affiliate species and genera, but even families and orders, and the frequent discovery of missing links offers the most striking testimony to the truth of the theory of Evolution.

That Phillips was thoroughly justified in his position towards Evolution is suggested by the fact that even Huxley, the most philosophic advocate of the theory, fully admitted that at the time of publication of the *Origin*, Palæontology lent to its doctrine no support.

An argument which evidently had great weight with Phillips, in his rejection of the theory of Natural Selection, was the excessive duration that it postulated for geological time. This still remains an argument of weight, so that some biologists impressed with the vast periods which the Darwinian theory demands for its operations, are prepared to measure geologic time by Darwinian requirements. On this subject Huxley expressed himself with his usual wisdom, perceiving plainly enough that biologists are in possession of no data as to the rate at which species may become modified—were we to judge the past from the present we might have to admit that they do not become modified at all—he referred biologists to the geologist, telling them in pithy words that they must take their time from the geological clock.

Phillip's position towards the Darwinian theory seems to me to have been altogether a wise one; since his time the doctrine of Evolution has obtained universal acceptance, but Darwin's theory is still a battle-ground for contending opinions.

And though we are compelled to call Evolution to our aid when we attempt to explain the facts of palæontology,

yet it is well we should bear in mind that Evolution does not advance us far. It is a great generalisation, as was the maxim that "Nature abhors a vacuum," but like this maxim it stands itself in need of an explanation.

Suppose by some ingenious cinematograph we could recall in a continuous picture the succession of vanished forms of life, and watch the transmutation of animals as they passed from formless protoplasm to man; the spectacle would be interesting, but would the fact it displayed be one whit less a miracle than that we witness every time we plant the seed in the ground and watch the green shoot spring forth, to produce in due time the secret buds which unfold in leaf and flower?

Or even suppose we take the one step farther and admit the Darwinian explanation, with its three factors of selection, variation and inheritance, are we much nearer to the ultimate truth? A certain Paley once considered a certain watch; the watch has grown so old-fashioned since Evolution attained its vogue that it is now rarely seen in polite society, but it did great service in its time; permit me on this occasion to draw attention to another analogy,—to the curious similarity that exists between the progress of human invention and the evolution of organic species. From the great kingdom of locomotive machines a lowly stirp arose known as the "hobby horse"; it lingered on unregarded for a while, and then was superseded by a new species technically called the "bone-shaker". This gained a place in the world, multiplied rapidly, and became represented by a great number of individuals; as it multiplied it gave rise by a process called "variation" to a number of related forms, such as the "ordinary bicycle," the "extraordinary," and the "kangaroo"; many of these species of machine exhibited a decided advance in organisation upon their ancestor; a process of selection then set in, the fittest types survived, the unfit became extinct; the good qualities, which had secured this success in life, were handed down by a process of inheritance; variation of these successful types produced others still more successful, and selection, always operative, continually weeded out the less fit, till

one sole survivor now remains, the summit of evolution is reached in the genus "bicycle," species "safety," variety "pneumatic"! Just such a kind of succession may be traced in the series of animal remains preserved in stratified deposits, a few simple forms appear, drag on a lowly existence for a while, and then unexpectedly blossom out into an infinitude of varieties; with lapse of time these become thinned out, but organisation all the time advancing there results at last some happily endowed form, which supplants the inferior competitors, and becomes lord and master of creation.

In the case of the machine we think we know the causes of variation and inheritance, we attribute them to the action of the human mind. In the case of the organic world, we assert that we know not to what these processes are due; and yet, somehow, to my thinking there stands behind them, connected in some incomprehensible manner, but still connected, the working of a mind which is Divine.

Professor Phillips was succeeded by Professor Prestwich, the Nestor of English Geologists, who maintained the principle of Evolution in Geology, as opposed to Uniformitarianism; and to him followed Professor Green, whose loss is still fresh and still deplored.

The influence of Oxford on Geology has been far greater than in this imperfect sketch can be made to appear: she has never shut her eyes to a new theory but, whether by criticism or advocacy, has endeavoured to 'stablish the truth that it might prevail; her direct contributions to knowledge have been very many and very great; if we mentioned no other work but one; that of *The Principles of Geology* by the immortal Lyell, would alone suffice to vindicate her fame.

W. J. Sollas.

ON PROGRESS IN THE STUDY OF VARIATION.[1]

(Continued.)

WE have now to examine the special case of local varieties or races, and to consider how the phenomena they present may best be turned to account in the attempt to investigate objectively the origin of species. It is here that we are especially dependent on the efforts of the collectors. Without great collections little progress can be made, but by modification of the usual practice their utility for the solution of these problems might be much increased.

It is a fact familiar to every naturalist that in very many species individuals living in different areas are dissimilar, and that by these dissimilarities the species may be broken up into local races. This phenomenon of local differentiation is so common that species in which it is not in some degree apparent may almost be regarded as exceptional. The differences may be exceedingly slight, and appreciable only to a person who has had long experience of the form in question; or, on the other hand, they may be so decided that it is only after special study and examination of series of specimens from many localities that the local races can be recognised as belonging to the same species.

In addition, too, to cases of this latter order, in which reason has been found for uniting dissimilar local races, there are numberless instances in nearly all orders of animals and plants where it is practically certain that forms which, on account of their dissimilarity and distribution have been considered as distinct, might with almost equal propriety be regarded as local races of the same species. Local differentiations of this kind are the despair of the systematist. How may species be distinguished from local forms? How great must the differences be, and in what organs must they ap-

[1] Corrigenda to part i. of this article, which appeared in the last volume of SCIENCE PROGRESS.

P. 556, line 3; *for* "most subalpine" *read* "most southern".

P. 556, line 7 from bottom; *delete* "wings".

P. 565, line 4; for "*monacha*" read "*eremita*".

pear, to justify the constitution of a species? On these weary questions volumes of controversy have been spent. No general rule can be found, for no real distinction exists, and in practice each man must follow his own standard. So long, however, as each distinct form receives a separate name by which it can be known and treated of it matters perhaps little whether it is reckoned as a species, sub-species, "representative species," local race or variety. All that we have to remember is that these various terms have no precise signification.

The existence of such local forms is nevertheless one of the best possible points of departure for an attempt to study the origin of species. In each case, where two local varieties are known, the problem of the origin of species is presented in a *particular* form. How did those two particular varieties A and B come into existence?

Causes apart, by what steps in descent was the one produced from the other, or both from something else? These are nascent species if there are any on the earth. Here if anywhere is our chance to see, if not the mode by which species come into existence, at least the way in which differentiated forms are connected together, and the steps by which they may separate. It is true enough that even in the case of most local races which are materially distinct the evidence as to the connecting steps is gone. In one locality one form is found, in another locality another form. Each area is distinct and isolated, and has its distinct population; beyond that nothing is known. But besides these there are other cases, rarer perhaps, but still fairly numerous, in which local forms inhabit conterminous areas, and, though distinct enough in their chief habitats, yet meet each other and breed together in the intervening district, producing offspring which we have no reason to suppose infertile. Such cases have a prerogative claim on the notice of the evolutionist. Two forms, each well defined, each, as is presumed, adapted by its peculiarities to its own area, meet in an intervening area. In what state is that intervening population? On seriation does it appear that the population consists mainly of one normal

form, the mean between the two others, or is the intervening population found to be practically divisible into two groups of individuals, the one more like the one race, and the other group more like the other race? In any such case there is in fact an opportunity of seeing "swamping" by intercrossing, of getting evidence as to whether the two races are capable of freely blending, or whether there is any discontinuity between them.

In regard to some of the more familiar examples, certain of which are spoken of below, discussion has arisen on the question whether the population occupying the intermediate area where the two races intergrade should be regarded as hybrids between the two, or rather as a still undifferentiated population. A third possibility is that the one race is being or has been directly formed from the other by discontinuous variation, and that in the area of intergradation the process is going on. On consideration, however, it will be seen that, whichever be the true account, the discussion must be a barren one; for if there is discontinuity between the two forms, whether the two races are newly met or newly dividing the appearances would at any one point of time be the same. This question, like so many others concerning evolution, could only be answered by appeal to observations made at several moments separated by intervals of time. Such evidence is, of course, wanting. Nevertheless, for our purpose a knowledge of the truth of one or other of these views is not immediately needed. The essential fact is that the two forms, though distinct enough to pass for separate species, did they not occupy the same area, on overlapping interbreed in nature. Though in the ordinary sense of the term these forms are not "species," yet they have many of the attributes of species.

If it is true that the evolution of the one form from the other, or of both from something else, has proceeded by a long series of insensible steps, of which each in turn has been a normal in its own day, surely we should expect to find the intermediate area occupied by an intermediate population, having an intermediate (if not the mean) for its nor-

mal. More especially should we expect to find this state of things in those instances in which the two local races are what are called climatic varieties, varieties that is to say associated with conditions which we can recognise as distinct (without postulating any direct relation of cause and effect). It is not rare, for instance, to find a species represented in the North by a northern race, and in the South by a southern race. In such a case, if the two forms inhabit a continental area, divided by no natural barrier, we should expect, on the hypothesis indicated, that in the intermediate area there should be an intermediate normal. Or, more precisely, we should expect in travelling from North to South to pass through country inhabited by a whole series of normals, passing in unbroken continuity from the distinctly northern form to the distinctly southern.

Again, if a lowland species is represented in the mountains by an alpine race we should expect as we travel up the valleys to pass through a series of normals, each appropriate to its own level. We may concede that a full chain of intermediates as normals need not be expected in every case, but that the state of things ought, on the whole, to bear out this expectation is surely clear, and if the hypothesis of an essentially gradual evolution is true, the geographical transition from the one race to the other should be essentially a gradual transition.

It is very likely that cases could be produced where such essentially gradual transitions occur, but, as will be shown, there are others which cannot readily be so described. Whether there are any general features distinguishing the two classes of cases we cannot yet say. It is the object of these remarks to call attention to the paramount importance of such phenomena as subjects for investigation.

Few indeed are the instances which we can yet with confidence refer to either class. Collectors and systematists have hitherto been content as a rule with the bare knowledge that there is sometimes intergradation between local races. Statistical evidence of the modes of intergradation are almost entirely wanting. Commonly the information is of the most meagre description ; and even in the case of

easily accessible forms no collections or experiments have yet been made with a view to answering a question of such peculiar interest to the evolutionist.

In the following pages I propose to take a few illustrations—most of them already well-known to specialists—showing the kind of phenomena which are thus open to observation. The methods that should be applied are essentially the same in each case, consisting in :—

(1) The collection of large samples taken at random in the area believed to be occupied by the pure races.
(2) Separate seriation of the individuals of each sample according to the degree in which the differentiating character is presented.
(3) Similar collection and seriation of similar samples from a series of stations connecting the areas occupied by the pure races.
(4) In suitable cases experimental crosses between the pure forms and between each pure form and such intermediates as can be obtained.

It is unnecessary to say that in planning observations on these lines, special regard must be paid to the possibilities of error due to migrations and other sources of error affecting particlar cases.

Local races may be distinguished by differentiation in respect of various bodily features, though naturally the most familiar are those which are distinguished by some conspicuous and easily recognised characteristic, such as peculiar stature or proportions, colour, sculpture, and so forth. But there are several well-known instances where intergradation occurs between races distinguished from each other by what are called "anatomical" characters. A good instance of such a phenomenon is that of a species of *Cistudo* in N. America, which is represented in New England by a form with four toes on the hind foot, while Mexican representatives have three. This form was originally considered by Gray as a separate genus, afterwards as a separate species. Subsequent evidence has shown that the fourth toe fades away so gradually that the

two forms are clearly one species. Specimens have also been seen with three toes on one foot and four on the other.[1] These animals are so common that a statistical study of their variation in nature should not be very difficult to make.

Among birds some of the most striking examples are known, and have been the subject of a good deal of discussion which, for the most part, has followed exclusively the lines indicated above. One of the best known is that of the species of *Colaptes* (a Woodpecker) inhabiting the United States and Mexico. In this instance the facts have been examined in considerable detail, especially by J. A. Allen, (1) who has given a careful summary of the evidence derived from the study of a large mass of material. As appears from his observations the case is briefly this. Omitting forms with more restricted range, two species of *Colaptes*, *C. auratus* and *C. cafer*, occupy nearly the whole of N. America. A band of country 300 to 400 miles wide extending from British Columbia to Texas is common to the two forms. North and East of this belt *C. auratus* is found unmixed, while *C. cafer* in the pure form occupies most of the country south and west of the common area. The distinctive characters are very striking, the most noticeable being the following :—

C. auratus.	*C. cafer.*
1. Quills yellow	1. Quills red.
2. Male with black malar stripe.	2. Male with scarlet malar stripe.
3. A scarlet nuchal crescent in both sexes.	3. No nuchal crescent in either sex.

Besides these there are several less conspicuous differences, one of the most singular being due to the fact that the colours of the crown and throat are *transposed* in the two forms, *C. auratus* having a grey crown and a brown throat, while *C. cafer* has a brown crown and a grey throat.

In describing the "ever-varying combinations" of these characters found in the area where the races overlap Allen states that the specimens range from individuals which show

[1] I have referred to the evidence in detail, *Materials*, No. 609.

the characters of one race with only a trace of the other to birds in which the two sets of characters are about equally blended :—

"Thus we may have *C. auratus* with merely a few red feathers in the black malar stripe,[1] or with the quills merely slightly flushed with orange, or *C. cafer* with either merely a few black feathers in the red malar stripe, or a few red feathers at the sides of the nape, or an incipient, barely traceable scarlet nuchal crescent. . . . The quills may be orange yellow or orange red, or of any shade between yellow and red, with the other features of the two birds about equally blended. But such examples are exceptional, an unsymmetrical blending being the rule, the two sides of the same bird being often unlike. The quills of the tail, for example, may be part red and part yellow, the number of yellow or red feathers varying in different individuals, and very often in the opposite sides of the tail in the same bird. The same irregularity occurs also, but apparently less frequently, in the quills of the wings. . . . A bird may have the general coloration of true *cafer* combined with a well-developed nuchal crescent, or nearly pure *auratus* with the red malar stripes of *cafer*. . . . Or we may have the general plumage as in *cafer* with the throat and crown as in *auratus*, and the malar stripe either red or black, or mixed red and black, and so on in almost endless variations, it being rare to find, even in birds from the same nest, two individuals alike in all their features of coloration."

Now, though from this account it appears that the several characters may combine in varying ways, the great irregularity and especially the asymmetry of the combinations are strong indications that there is not free blending of the characters, but rather that though they may co-exist in the same individual they are in some degree alternative, forming, in fact, a kind of patchwork. Though even in this—one of the best known cases—adequate statistics are still wanting, it seems to be clear that there is no great population with either the mean or any other intermediate form as a definite normal.

A more complex example is provided by species of the genus *Quiscalus*, the Boat Tail Grackle of N. America. The facts are indeed so complicated that they cannot be represented in a brief statement. A full account of this matter is given by F. M. Chapman (2) who has made as far as

[1] A Californian specimen in the Cambridge Museum shows these feathers partly black and partly red. Each feather though black in its basal parts has a red tip, the black pigment being apparently transmuted into red pigment.

his material permitted, a statistical tabulation of results. Here again the variation is in colour of plumage, which may take various dark metallic shades not easily described in words. Concomitant variation in size and proportions are also shown to occur. The facts as stated by Chapman are, broadly speaking, that *Quiscalus æneus* has a breeding area from the Rio Grande Valley northwards to British America, and north-eastwards to New Brunswick. In the area south-east of this *Q. quiscula* occurs, ranging from Florida to Massachusetts. In Pennsylvania and Massachusetts (probably also in a belt of country extending south-west from Massachusetts through Pennsylvania to the north of the Mississippi) the two forms meet and intergrade. In the rest of its range *Q. æneus* is remarkably constant. The complexity arises from the fact that *Q. quiscula* occurs in a variety of forms which can be regarded as three phases of coloration. Phase No. 1 is found in S. Florida associated with Phase No. 2, and many intermediate forms. At the north of its distribution Phase No. 1 is absent, No. 3 being there chiefly found, associated with No. 2 and all intermediates. In the central part of its range all three phases and intermediates occur together. The transition from *æneus* to *quiscula* occurs exclusively through phase No. 3 of the latter.

From the particulars given it seems likely that though the transition is gradual in the sense that all intermediates occur, being indeed frequent in the area of intergradation, yet here again the connecting links are not a series of normals following each other in succession along lines passing from the area of one race to that of the other. Another example well known to ornithologists is that of the Rollers, *Coracias indicus* and *affinis* (see Dresser (3) and Sharpe (5)). Here again the coloration of the two races or species is quite distinct. *C. indicus* inhabits the Indian Peninsular generally, extending westwards to Asia Minor, while *C. affinis* belongs to British Burmah and Indo-China. It spreads, however, westwards to the neighbourhood of Calcutta and onward to Sikkim and Nepal. This part of its distribution is common to the two forms, and intergrading

occurs. A considerable series of intermediates from this region is preserved in the British Museum, and from these and others described by Dresser it appears that here again there is no definite race of intermediates grouped round the mean as a normal, but that the intermediates are a heterogeneous body, and highly variable. It is quite possible that on seriation they would be found to be on the whole divisible into two groups favouring the two types respectively.

Birds, however, are not well adapted to statistical treatment of the kind contemplated. Their large size, frequently migratory habits, and the comparative difficulty in preserving them in large numbers makes them for the most part unpromising material for such work. Besides this, it is in very exceptional cases only that they can be bred in captivity. Before passing to other examples, however, it may be noted that in these cases, though the individual parentage of the intermediates or of the types in the area of intergradation is scarcely as yet a matter of observation, there is nothing in the facts inconsistent with the possibility that the whole phenomenon may be one of discontinuous variation. Nevertheless, it is quite as possible that the intermediates may in each case be the result of cross-breeding. If the latter is the true view, then, since the whole population is not found to have regressed to the mean form, this state of things must be due either to the existence of some principle by which the types mate more often legitimately than illegitimately, or to the existence of natural discontinuity, or to the unfitness and consequent extermination of the mean forms. The last hypothesis is so improbable that it need scarcely be considered. No support could be produced in behalf of such a view, either from facts of the case or analogy of other cases.

Among Lepidoptera there is perhaps more hope of obtaining direct evidence on this question, and there are numerous cases which would well repay careful work. Scarcely any have been touched as yet.

A remarkable example has been observed by W. H. Edwards (4) among the N. American Papilios. The form

known as *P. oregonia* is a species much resembling *machaon* in general coloration. It is found in Washington Territory and British Columbia. Another "species," *P. bairdii*, has the upper surface largely covered with black scales, especially in the female, and in general appearance may be said to resemble the *asterias* group more than the *machaon* group. It occurs in Arizona without intermixture with *oregonia*, just as the latter occurs in the north without intermixture with *bairdii*. However at Glenwood Springs in Colorado *oregonia* and *bairdii* were found flying together by Edwards.

By rearing eggs laid by the Glenwood females, he found that both forms could be reared from females of either form, eggs from *bairdii* females producing both *bairdii* and *oregonia*, and eggs from *oregonia* producing both *oregonia* and *bairdii* (statistics given). The experiments showed that this is not merely a case of seasonal change, both forms occurring in each brood.

For our present purpose it is important to notice the fact to which Edwards calls attention that the *oregonia* and *bairdii* in the locality of intergradation were not for the most part the pure forms as they occur in areas where there is no intermixture. Some of the *bairdii* were typical, but most departed in different degrees from the type, no two being quite alike. The *oregonia* also departed from the type in the direction of *bairdii*. Nevertheless as the whole account shows, here there were two well-marked forms like those elsewhere known as local species or races, breeding together in one locality but not regressing to the mean form.

I will now refer briefly to two examples of marked local races, whose relations I have been myself endeavouring to investigate. The cases are those of *Pieris napi* (the Green-veined White Butterfly) and its Alpine var. *bryoniæ*, and *Pararge egeria* (the Speckled Wood Butterfly) and its northern form *egeriades*. I have been collecting and crossing these species for the past three seasons, and am still far from arriving at definite results, but I mention the subject here in the hope that others may be induced to look out for some of the points to be observed.

As regards these cases the history is far too complex to be treated as a whole in the present paper. Difficulty arises not only from the great extent of country inhabited by the species and their varieties, but also in the case of *napi* from seasonal dimorphism. *P. napi* in this country is too well known to need description. Here as a rule the summer brood do not differ greatly from the spring brood, though exceptionally they may do so. The summer form when it is distinct is called *napææ*. It is chiefly characterised by the large size of the two spots on the fore wing in the female, and by the great reduction in the amount both of dark venation on the upper surface of the fore wing, and of green venation on the lower surface of the hind wing. Such a distinct summer form abounds in the lower parts of the valleys in the Alps and of course in most of the warmer regions of Europe.

At and above 3000 feet or rather less, *bryoniæ* may often be met with in those valleys which descend from the high mountains. It is in the *female* that this variety is clearly marked, being characterised by the presence of bands of brown scales following the nervures or the upper side of both wings, especially in the fore-wing. The male closely resembles the spring male of *napi*, and is not generally, if ever, distinguishable from it with certainty. *Bryoniæ* extends up to 6000 feet. Whether this alpine form ever occupies any considerable area without *any* admixture of *napi* I do not know. As regards particular hillsides I think it does, but my experience is that colonies of *napi* may occur up to nearly 6000 feet, especially near chalets, appearing in summer broods identical with those of the lowlands. That in such places the two forms occupy more or less distinct spots, is, I think, probable.[1] Lower down, however, *napi* and *bryoniæ* may in many places be taken flying freely together, but in such localities, according to my experience, intermediates are of exceptional occur-

[1] It is possible that the two forms in such cases live on different plants. The chief food of *bryoniæ* at Tosa is *Biscutella lævigata*, but both forms eat many kinds of Cruciferæ, and I cannot find that the colours are changed by difference in food.

rence and are less common than the type forms. Why is this?

The case is an interesting one in several ways. Weismann has made this species the subject of a good deal of speculation and some experiment. In his earlier paper (6) he regards it as certain that *bryoniæ* is an ancient form dating from the glacial period and that *napi* has been very gradually evolved from it as temperate conditions supervened. In the later paper (7) from the fact that two male *napi* and a *bryoniæ* female irregularly patched with white emerged from some forced pupæ supposed to be all offspring of *bryoniæ*, he is apparently disposed to doubt his original view, though, as he points out, the *napi* may have been merely introduced larvæ and the blotched specimen may have been gynandromorphous. The possibility that there may be discontinuity is not considered. Moreover, the matter is greatly complicated by the fact that *bryoniæ* is not, as Weismann states, exclusively one brooded, for I have reared summer broods, emerging in August, both from *bryoniæ* females caught at Fobello (3000 feet) and above the Tosa Falls (5500 feet). The former were flying with *napi*, and for anything I know these summer emergences may have had a *napi* father.

Those from Tosa may also have been cross-breds, though this is less likely. All the second brood from Fobello were intermediate and some of those from Tosa are so also, though the rest of the latter are *bryoniæ*. It appears therefore to be possible that the intermediates found wild may be a second brood from *bryoniæ*. In order to determine this it would be necessary at least to have statistics from one locality extending over the whole period during which the species fly.

The view that *napi* has been very slowly evolved from *bryoniæ* or *bryoniæ* from *napi* is not easy to reconcile with the facts of the present occurrence of the forms. *Bryoniæ* is met with in the far North and again in the high Alps. If *napi* has been continuously evolved from it, passing through a long series of intermediates as normals, we should expect that those places in Europe which have a climate inter-

mediate between glacial conditions and temperate conditions would have a normal population of intermediates. On such points the systematic treatises are not very reliable; but I cannot discover that such a population exists anywhere. With regard to Norwegian and Arctic specimens the accounts are somewhat contradictory, and the individuals I have seen are too few to warrant a definite opinion. The same applies to Central Asian forms. Certain it is that in the Alps on passing from the lowlands to alpine conditions a population having an intermediate as its normal form is not met with in June and July. At this time in such localities the common forms are *napi* of the second brood, *bryoniæ*, and occasional intermediates which may be either pure-bred second brood *bryoniæ* or cross-breds, or both.

It is to be hoped that some who have the requisite opportunities will make collections of large samples of these forms, either in the North of Europe or in alpine regions, and so contribute to the solution of this very attractive problem. It will be understood that the account here given is the merest outline of the facts.

It is interesting to reflect that if we had known nothing of the history of the origin of *A. betularia*, var. *doubledayaria*,[1] and if we had not actual historical evidence that it has replaced the type in the North of England we should most certainly have been told that the one had been very gradually evolved from the other in the course of ages. The same example shows moreover that there need be no natural distinction between the case of a variety discontinuously occurring with the type and the case of two local

[1] To the experimental evidence of discontinuity in this case I should have added the following reference, E.R. Bankes, *Ent. Rec.*, vii., p. 181. Since the first part of this article was published, Mr. W. H. B. Fletcher has kindly lent me the entire brood produced from a black female, reared in captivity, which was tied out at Worthing where only the type is known. There can thus be no doubt she was fertilised by a *betularia* male. The offspring are sharply divided as follows: *betularia*, ten males, eight females; *doubledayaria*, six males, five females. Curiously enough one of the *betularia* is an abnormally light specimen. The rest are either normal, or else completely black all over.

races occupying distinct areas. We might suppose that the former case is in reality comparable with familiar examples where there is an apparently constant dimorphism of one sex, or with the reciprocal dimorphism of many flowers, in which it is believed that the dimorphism is a permanent attribute of the species as much as the differentiation of parts in the body of an individual. The case of *betularia* shows how the one may pass into the other.

Before leaving the subject I will give one more example, to which I have been paying some attention, because it is especially one where co-operation is needed. I take this opportunity of thanking M. René Oberthür for kindly sending me specimens, and for other assistance rendered me in regard to this species. The Speckled Wood butterfly of England is now well known to be a geographical variant on the form inhabiting France and Spain generally, the Mediterranean littoral and a great part of the rest of Europe. In the South the pale yellow of our own insect is replaced by a bright fulvous yellow. The southern form is now called by the strict rules of nomenclature *Pararge egeria*, while the British form is the var. *egeriades*. Omitting other complications, our problem is raised in the simplest form by the facts of the distribution of the varieties in the French and Spanish continent. At Gibraltar *egeria* flies alone. Practically similar *egeria* flies alone in the Basses Pyrenées (as also at Avignon and Tarascon) and in all the country northwards as far as Poitiers. About sixty miles north of this point, at Tours in the Loire Valley, country not noticeably different, there is a very different form which is almost exactly intermediate between the two chief varieties. This intermediate form is spread over Brittany and Normandy. Those from the neighbourhood of Paris are of the same intermediate form, or perhaps a little paler. As far as my own observation goes. there are essentially *three* forms, southern, northern and intermediate. From between Poitiers and Tours I have no specimens, and it would be interesting to know whether in that region the southern form shades off into the intermediate. Similarly it would be interesting to have col-

lections from places where the intermediates might be expected to shade off into the northern form.

It is certainly likely that there may here prove to be a true case of continuity, but so far this is not quite clear, either from the wild specimens or from those that have been reared by crossing. The cross between the northern and southern types usually produces the intermediate, agreeing exactly with Breton specimens, but on recrossing these with the southern form my evidence inclines, but by no means certainly, towards the conclusion that there is not complete blending between the two. I anticipate, on the whole, that fuller investigation will show that there is complete or almost complete continuity. The abrupt change on passing from Poitiers to Tours is, however, scarcely consistent with this anticipation.

The insect is very local, and probably wanders hardly at all. It is therefore a good subject for this inquiry. The west side of France is most suitable ground for the investigation, for here there are no mountain ranges or other barriers.

In mountainous parts of the country the true *egeria* is always replaced by paler forms. Such forms are found, for example, at Chambéry, and these are not strikingly different from those of Brittany, though on the whole inclining to the fulvous type, while from Doubs M. Oberthür has sent me specimens precisely like English ones.

Cases which might repay statistical study in this country are, amongst others, those of the varieties of *Polyommatus agestis* and the two forms of *Fidonia piniaria*. The latter is very probably a case of discontinuity. In examining it difficulty may arise, as Mr. Fletcher has pointed out to me, from the possibility that with the recent plantation of the Scotch Fir in many parts of the country artificial introductions may have taken place. I should be grateful for the loan of series of specimens or for accurate records of the occurrence of these forms.

The illustrations here given will suffice to show lines on which the objective study of the origin of species may, as it seems to me, be profitably pursued. Until work of this

kind has been carried out in many cases, both statistically in the field and by cross-breeding in captivity, we have not got the material even for a preliminary survey of the relations between species and variety. Such work is pre-eminently to be commended to collectors and systematists. It is for want of it that so little progress has been made with these questions. It is depressing to see how those who are engaged in the business of systematic work often neglect to give the essential particulars as to the variability of the material submitted to them for description. That such a character is "variable," or "so variable that no reliance can be placed on it" is often all that we are told, when in many cases with little additional trouble the number of specimens exhibiting each variation could have been recorded, thus greatly lightening the task of those who come after. If collectors and systematists would arrange their work in such a way as to bring out and not conceal the objective phenomena of evolution, and if the evolutionist would appreciate that the proper way to study the relation of type and variety is to take up the work at the place where the systematist leaves it, we should have that partnership between the two classes of naturalists for want of which so much effort is wasted and progress is so slow.

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W. BATESON.

PREHISTORIC MAN IN THE EASTERN MEDITERRANEAN.

PART II.

THE MYKENÆAN QUESTION.

45. A RECENT article summarised the evidence of the course of the dominant civilisation in the Eastern Mediterranean down to the point where Hellenic history may fairly be said to begin. But the caution is perhaps hardly yet superfluous, that community of civilisation does not prove community of race, though it frequently accompanies it, and sometimes can be shown to result from it. The question therefore which next occurs is twofold: firstly, from what source or blended sources is the Ægean civilisation derived; secondly, if it is indigenous, what is the ethnographic position of the race or races who originated it; and if it is immigrant, who, in addition, introduced it, and from whence, among the peoples of the Ægean area and their neighbours.

46. A premature but not unnatural conjecture led Dr. Schliemann to equate absolutely the civilisations and peoples of Mykenæ and what we know now as pre-Mykenæan Hissarlik with those described as Achæan and Trojan respectively in the Homeric poems; and a large part of the subsequent commentary on these and kindred discoveries has approached the question from the side of the Homeric literature, and has turned upon the validity of this identification. But this identification itself only substituted an Achæan *y* for the Mykenæan *x*, and the problem still remained of the relations in which Homer's Achæans stood to their namesakes in historic Hellas, and to the other Hellenic stocks.

47. Dr. Tsountas has attempted to dissever ethnologically the representatives of the Cycladic from those of the Mykenæan stage of Ægean civilisation, and to identify the former with the Danaans, the latter with the Achæans

of the Homeric poems, whom he regards as incomers from the north, and responsible in particular for the replacement of the Cycladic and early Mykenæan rite of burial in cists or shaft graves by the later Mykenæan usage of chamber burial.

48. The same conception of the Achæans as a dominant caste of immigrant northerners, superimposed on an aboriginal population who had the Ægean culture already in a high stage of development, has been amplified in special relation to the literary question by Dr. Leaf; and, as explaining the gradual acceptance of the Achæan tribal name Hellene as a national designation, by Professor Bury; while in identifying the pre-Achæan aborigines with the Pelasgians of Hellenic tradition Professor Ridgeway has perhaps succeeded in substituting, yet again, a Pelasgian *z* for the Achæan *y*; but perhaps also in the process has contributed in detail to the eventual determination of both unknowns.

49. But behind the Homeric Question, which is really the question of the relative date of the Homeric Poems themselves or their essential constituents, stands the main question at issue, how far the Mykenæan civilisation should be regarded as indigenous at Mykenæ and on other Greek sites; how far as having been imported more or less ready made. For the great differences between the Mykenæan and the Hellenic in almost every department of art and culture made a transition in direct series from one to the other at first sight almost inconceivable, especially in view of the admitted break both in the ancient Hellenic tradition, and, until lately, in every class of evidence for the intermediate period.

50. The fact that Dr. Schliemann happened at the outset, at Hissarlik and Mykenæ, upon two of the materially wealthiest Ægean sites hitherto known, and in the latter case upon one which until 1896 remained wholly unchallenged for the variety and magnificence of its remains, made the indigenous growth of a civilisation, with a character so pronounced and resources so ample, even more difficult to reconcile with preconceived ideas than it might have been if minor and sporadic discoveries had prepared the way

for the recognition of these masterpieces. And the new material appeared at a moment when literary and philological criticisms had in many quarters combined to weaken the authority of the Hellenic, and particularly the Homeric tradition of a Golden Age, of which Hellenism should be regarded as a Renascence. Consequently the ingenuity of archæologists has been largely directed rather to the indentification of foreign and imported elements, in the later stages of Ægean culture, than to the affiliation of these to an indigenous parentage, and to the conception of Ægean culture as simply that corner of a larger Mediterranean and European culture which was most favourably placed for expansion on its own lines, but not to be regarded as distinct from that larger whole, simply because in its later stages it had so expanded.

51. The large crop of theories of Ægean origins may be roughly divided into three groups. If Ægean civilisation was not mainly indigenous—and this was for a while the prevailing impression—it must either have been derived from an Asiatic, or an African source. If from an Asiatic, two routes seemed to be open, by the land route of Asia Minor and the eastern coast of the Ægean, or by the Syrian and Phœnician coast; these being the two main lines of communication with the Euphrates Valley which remained open and in use at the beginning of Hellenic history. If on the other hand from an African, which before the detection of an early Libyan civilisation meant an Egyptian source, then the route must either have been again *viâ* Phœnicia, or more directly along the north coast of Africa; but in spite of Homeric indications, the latter does not seem to have been seriously considered until quite recently. To take the Anatolian theories first and then the Syro-Phœnician:

52. The "Karian Theory" of Drs. Köhler and Dümmler rested upon Hellenic traditions of an occupation of many of the islands, some part of Crete, and several points of the mainland of Greece, by Karians, or according to another version by Leleges from Karia. This thalassocracy had been succeeded by the Cretan thalassocracy of

Minos, whom Hellenic tradition placed at least three generations before its Trojan Era. The expelled Karian, or Lelegian occupants of the islands had been driven back by Minos into Karia and survived in the adventurous seamen who shared with Ionians in the rediscovery of the outer Mediterranean in the seventh century, B.C. Drs. Köhler and Dümmler identified Karian and Mykenæan, and placed the centre of origin of Mykenæan civilisation in Karia, which was then (1886), still unexplored. Recent investigations on the Karian coast however seem to show that Karia, so far from originating the civilisation in question, remained unreceptive of it until it was already in its decline in Hellas and the Ægean: and that such slight traces as occur are more probably referable to settlements of Ægean (probably early Hellenic refugees) in the catastrophic period which closes the Mykenæan Age.

53. The discovery in Upper Phrygia of rock-cut tombs of early date, with heraldic representations of lions and other figures in archaic style above the entrance, gave ground for a theory of a Phrygian, or partly Phrygian origin for Mykenæan art: especially in view of certain traditional and ethnographic correspondences between Phrygia and early Crete. The dominant race in Phrygia in the eighth and succeeding centuries was regarded, and with some probability, by its contemporaries as of near kindred with one of the strains of the composite Hellenic race. But the most allied strain seems—though this is not clear—to be of late arrival in Hellas; and it has also been shown by more careful examination, and on fuller evidence than at first, that the Phrygian sculptured tombs are of date not earlier than the eighth century, and in many cases appreciably later; so that the evidence points, as in the case of Karia, the other way if at all.

54. Another theory of an origin of Ægean civilisation in Western Asia Minor is that of Dr. Montelius, who regards both the Mykenæan and the Etruscan culture and peoples as representatives of a great emigration westwards from the hinterland of Lydia and Karia. This view lays extreme emphasis on certain Syro-Kappadokian (Hittite)

and Assyrian influences which have been detected in the Mykenæan art, and on a body of Hellenic tradition which represented the Etruscans (Tyrrhenians) who in any case appear to have reached their Italian home by sea, as ethnologically related to the Lydians. Excessive stress also is perhaps laid on the points of contact between Mykenæan and Tyrrhenian civilisation. Mykenæan importations, and imitations of them, are not infrequent in Etruscan, as in Sicilian and Venetic tombs; but there is no clear break at the beginning of the Mykenæan series in Italy such as would be required by the theory of a general immigration of the representatives of an Anatolian culture. Moreover, the complete absence (with the exception of Hissarlik) of Mykenæan city-sites, and the excessive rarity even of single Mykenæan objects on the Anatolian coast, are as fatal to this as to the Karian theory already mentioned.

55. Yet another variant of the Anatolian theory makes Mykenæan art the product of early Hellenic settlers on the Asia Minor coast, under Phœnician and Orientalising influences. This view depends upon the assumption (SCIENCE PROGRESS, 1896, p. 353) that Mykenæan civilisation lasted on, even if it did not wholly arise, in the centuries after, rather than before the tenth; and stands or falls with that chronological theory. It is a further obstacle to its acceptance, that, as already noticed, Mykenæan objects have at all events hitherto eluded observation on the sites of all the Anatolian towns, and appears to be confined, on this side of the Ægean, to the islands which fringe the coast; again with the exception of the late or sub-Mykenæan objects which have been found in half-barbarous native tombs at Assarlik, Mylasa, Tschangli and in the neighbourhood of Sardis.

56. A Phœnician theory of the origin of Mykenæan civilisation was of course early in the field. Hellenic tradition had accepted a Phœnician thalassocracy over almost the whole Mediterranean at a period subsequent to that of Minos; an extensive Phœnician commerce in the Ægean as elsewhere, and Phœnician settlements on many points of the Ægean coast: and the immature philology,

archæology and comparative mythology of the first half of the present century found reason to extend the range of Phœnician influences even beyond the very wide limits of the Hellenic view.

57. In spite of the almost complete absence of data for the history of Phœnicia and Phœnician art before the eighth century, B.C., which still confronts us; of the almost uniform divergence of the style of such objects as can be identified as early Phœnician from that of their Mykenæan counterparts, and of the growing mass of evidence that like Karia, Phœnicia, and the whole Palestinian area, borrowed more than it lent, from this, the only living school of culture in the Mediterranean during the period in question, repeated attempts have been made to show that Mykenæ, Ialysos and Gnossos represent, not native Ægean centres, but trading stations or colonial settlements of Semitic adventurers and emigrants established amongst relatively barbarous aborigines.

58. The theory if fully developed would be in a position to take into account the existence of earlier stages of development within the Ægean itself, for Hellenic tradition maintained that these and other islands received Phœnician or "Kadmeian" colonies at a very early date. But it has been most recently treated, by an authority high in another department, in the light of a half knowledge which would seem to ignore these earlier stages altogether, and to treat fully developed Mykenæan art as a newly arrived importation, the earlier stages of which would still be to be sought elsewhere than where they had actually been found ten years previously. The same essay touches also the Etruscan question, and treats Mykenæan imports in the West as further evidence of the œcumenical range of Phœnician commerce.

59. The argument that, as all known Phœnician objects are subsequent to the probable date of the Mykenæan age, they cannot well be the prototypes of Mykenæan art has been ingeniously met by MM. Pottier and Helbig by claiming Mykenæan art as being itself, either wholly, or at least as regards its masterpieces, the long-lost "Sidonian"

art of an earlier Phœnician period known to Greek tradition, but hitherto unrepresented either by monuments in Phœnicia itself, or by undisputed identifications elsewhere; for the identification of the Keftiu of Egyptian monuments with the Phœnicians is open to grave objections; and even if it be accepted, it still remains to find any trace of anything but the latest Mykenæan decadence on the Syrian coast; any proof, that is, that Mykenæan, not to mention Cycladic, objects were ever manufactured by the Phœnicians or their neighbours, or that the offerings of the Keftiu are not themselves rare and valuable importations from the Ægean art-centre.

60. The argument, too, from the voluminous Hellenic tradition of a Phœnician thalassocracy would itself fall to the ground if the very probable hypothesis became demonstrable, that Hellenic anthropology also had laboured under a *Mirage orientale;* and that the un-Hellenic and pre-Hellenic monuments and other objects which came to light in Hellenic times were interpreted as products of an immediately prehistoric stage of the commercial system which historically prevailed until it was superseded by Hellenic enterprise. Nothing is more striking, in fact, in the speculations of Hellenes, from the sixth century onwards, on their own origins, than their inclination to refer every advance in every department of their civilisation to foreign initiative; and it has to be remembered that, as above stated, Phœnician trade may be admitted to have been predominant in many parts of the Mediterranean in the blank period which intervenes between the Mykenæan and the Hellenic thalassocracies; and that consequently it was actually through Phœnician mediation largely that the earlier Hellenic antiquaries obtained from Egypt and Mesopotamia the material for comparison with their own prehistoric monuments.

61. Closely allied to the Phœnician theory is the Syrian or Syro-Kappadokian theory of Dr. Beloch, which would associate Mykenæan art with that style which it is still convenient to label as Hittite. The points of similarity, however, hardly justify the very wide inferences which have

been made from them; and are themselves in many cases quite explicable as independent loans from Egypt. Here, too, in the present state of the chronological evidence it must be left at least an open question whether the Mykenæan is not the prior member of the comparison.

62. Finally, in discussing certain features in Hellenic religion—one of the most complex religious systems of the world—M. Foucart has collected our fragmentary knowledge of Egyptian naval enterprise, with the object of showing that native Egyptian enterprise in the Ægean, independent of Phœnician trade, is admissible as a means of communication with the primitive Ægean; but, between proving too much and proving too little, the attempt cannot be said to have wholly succeeded.

63. In complete contrast with all the preceding theories, is the alternative view that the Ægean civilisation is in the main indigenous, and that such Anatolian, Phœnician, or Egyptian features as can be recognised are due not to the predominance of Oriental peoples or trade in the Ægean, but to the expansion of Ægean enterprise into the Levant. Even Hellenic tradition, so apt, as we have just seen, to under-estimate the originality of the civilisation of Hellas, bore witness to frequent marauding and trading visits of "Achæan" adventurers to the coasts of Phœnicia, Egypt and Libya: and the repeated occurrence of Ægean imports on Egyptian sites, especially of twelfth and eighteenth Dynasty dates, the contemporary Egyptian record (already referred to) of invasions of Egypt by "peoples of the isles of the sea" from the north-west and often apparently with Ægean names; and the persistence in early Hellenic times of a trade route from the Ægean *viâ* West Crete and the Cyrenaic country to the Nile delta, seem amply to confirm this traditional belief.

64. It is thus, in particular, possible to admit frequent communication with Egyptian civilisation without assuming the existence either of the Phœnician middleman, or of an independent Egyptian trade system: and it will be seen hereafter that an increasing mass of new evidence in regard to the civilisation and the peoples Libya itself has been thus

brought into line, which was practically ignored by the Anatolian and Phœnician theories, and which was in fact for the most part not yet available at the time when these were formulated.

65. Dr. Schliemann's brilliant results at Mykenæ and Tiryns of course gave the impression that the centre of gravity of the Mykenæan world lay on the western side of the Ægean basin, and within the area of the "Kalaurian league" of maritime towns recognised in Hellenic tradition. The settlement in Rhodes being regarded as probably an early colony from Argolis; again in accordance with the legend. Dr. Milchhoefer was the first to direct attention to the importance of the first instalments of very fragmentary Cretan evidence, and the magnificent series of discoveries of Mr. Evans in 1894-6 fully established the view that not only in the decadence and in the culminating period, but also throughout the earlier "Cycladic" stages, Crete exercised an influence more sustained and more dominant than any other section of the Ægean area: and in particular that, as has been noted already, it was in Crete that the pictographic means of written communication which prevailed was mainly elaborated and employed; while it is here too that, thanks to this island's comparative immunity from political and ethnic disturbances, the same script is most nearly approached by the earliest forms of the local Hellenic alphabets.

66. The question of the attribution of Mykenæan art cannot be said to be decisively settled by the evidence hitherto available; though the new material accumulated in the last four or five years in particular has been almost wholly in favour of regarding the Mykenæan objects known to us as products of Ægean industry (the only exception being the discovery in Cyprus of first-class Mykenæan sites whose relations it is not yet possible to estimate with certainty); and very largely also in favour of the theory last mentioned, that the civilisation itself, though undoubtedly affected by its mainly active and not passive contact with Egyptian, and to a less degree with Chaldæan civilisation, assimilated in a remarkable manner the motives and modes

of treatment which it borrowed, and that it can be shown to have developed by a spontaneous and uninterrupted growth, among a group of closely interrelated and indigenous populations.

67. The further question now arises, what is the ethnological position of the Ægean population themselves, and how far the recent anthropological and archæological evidence affects the validity both of the Hellenic tradition, and of certain ethnographic conclusions hitherto commonly accepted?

J. L. Myres.

(*To be continued.*)

METAMORPHOSIS IN PLANTS.[1]

WHEN I was invited to take part in these College lectures, it was suggested to me that I might advantageously take for my subject the Doctrine of Metamorphosis as illustrated by plants.[2] I have gladly adopted this suggestion, not only because I hope to interest you by a discussion of the methods and concepts of morphological Botany, but also because there is a certain appropriateness in addressing such an audience as this upon a subject to the development of which at least two distinguished philosophers and men of letters have largely contributed.

The questions which naturally arise at the outset are, What is the Doctrine of Metamorphosis? and To whom are we indebted for it? I think that I may well begin my answer to the first question by giving you a very brief sketch of the general morphology of a plant. You know from your own observation, and you may see by means of the diagrams and specimens now before you, that the body of the more familiar higher plants consists, speaking generally, of certain distinct and easily recognised parts, root, stem, leaves, flowers. The stem, or its branches, bears the leaves and the flowers; the root bears neither. Confining our attention to the flowerless or vegetative region of the

[1] This lecture was delivered to members of Magdalen College, Oxford, on 30th October, 1897.

[2] The following are some of the more important general works dealing with this subject, of which use has been made in preparing this lecture :—

Wigand, *Kritik und Geschichte der Lehre von der Metamorphose der Pflanze*, Leipzig, 1846.

Whewell, *History of the Inductive Sciences*, iii., 1857 (3rd ed.).

Herbert Spencer, *The Principles of Biology*, 1864-67.

Kirchhoff, *Die Idee der Pflanzen-Metamorphose bei Wolff und bei Goethe*, Berlin, 1867.

Lewes, *Life of Goethe* (3rd ed.), 1875.

Sachs, *Geschichte der Botanik*, 1875 (Engl. ed. 1890).

Goebel, "Vergleichende Entwickelungsgeschichte der Pflanzenorgane," in Schenk's *Handbuch der Botanik*, iii., 1883.

stem, we recognise a definite contrast between stem and leaf: the stem is axial in nature, the leaves lateral and appendicular. Turning now to the flower, we perceive that, unlike the leaf, it is not borne laterally upon the stem, but terminally on a branch. An analysis of the flower shows that it itself consists of an axial and of appendicular parts, the latter presenting widely different forms which are distinguished as the sepals forming the calyx, the petals forming the corolla, the stamens forming the androecium, and, finally, the carpels forming the gynæceum or pistil. The flower is, in fact, a shoot characterised by its limited apical growth, by its undeveloped internodes, and by the reproductive organs which it bears.

We are now in possession of a sufficient body of facts to enable us to proceed with an exposition of Metamorphosis. But before doing so I must point out that this term has not always been used in one and the same sense. It was first introduced into botanical language by Linnæus,[1] but he himself applied it to various phenomena; at one time to the flowering of plants, in which he finds an analogy with the change of the chrysalis of an insect into the butterfly; at another to the production of varieties and monstrosities; again, to the development of a plant from a seed or from a bulb; and, finally, he applies it (in the *Philosophia Botanica*), though vaguely, somewhat in the sense in which we are now about to consider it. It was not until forty years after its first use by Linnæus that the application of the term was clearly limited; and then, not by a botanist, but by a poet—Goethe, in the following words:[2] "(§ 1) Every one who in any degree observes the growth of plants will readily notice that certain of their external parts frequently show a change, assuming either entirely or to a greater or less degree the form of adjacent parts. (§ 2) For instance, the simple flower becomes a double one, petals developing in the place of stamens and

[1] *Systema Naturæ*, ii., 1735; *Philosophia Botanica*, 1751 (p. 301 in Willdenow's edition, 1790; p. 355 in Quesné's French edition of 1788); "Metamorphosis Plantarum" (Dahlberg) 1756, in *Amoen. Acad.*, iv.

[2] *Versuch die Metamorphose der Pflanzen zu erklären*, Gotha, 1790.

anthers; these petals are either exactly like those already present, or they retain visible signs of their origin. (§ 4) The hidden relationship between the various external parts of the plant, such as the leaves, the calyx, the corolla, the stamens, which develop one after the other and, as it were, out of one another, has long been recognised by investigators, and has indeed been specially studied; and the operation by which one and the same organ assumes various forms has been termed the Metamorphosis of Plants. (§ 119) As we have sought to explain all the apparently different organs of the budding and blossoming plant from a single one, namely, the leaf which usually develops at each node; so we have also ventured to derive from the leaf-form those fruits which firmly enclose their seed."

In these words the doctrine of Metamorphosis, as regards the appendicular organs of the plant, is clearly stated: all these parts are regarded as equivalent, whatever their function or their external form. Postponing for the moment the discussion of this generalisation, we may pause to consider the method by which it was arrived at, as also the labours of other workers in the same field.

In addition to Goethe's, there are two names which must always be mentioned when the subject of Metamorphosis is under discussion, the names of Linnæus and of Wolff. With regard to Linnæus, Goethe recognises in him one who had gone far along the same road as himself, but who had failed to attain so definite a goal. Linnæus, it must be admitted, had the sense of the equivalence of the different appendicular organs, but he failed to express it in the clear philosophic form which is the characteristic of Goethe's exposition: with him the idea was implicit, not explicit. The first suggestion of it is to be found in his *Systema Naturæ*,[1] where the following passages occur: "Prolepsis sistit Metamorphoseos Plantarum mysterium, quo Herbæ *Larva* mutetur in Declaratam Fructificationem. . . . Florem dum producat Arbor, Natura anticipabit quinque annorum progenies, simul tum produturas, formando e foliis gemmaceis

[1] *Systema Naturæ*, ii., 1735; I quote from the thirteenth edition, 1770, p. 8.

futuri anni *Bracteas*, sequentis *Calycem*, insequentis *Corollam*, consequentis *Stamina*, subsequentis *Pistillum*, refertum Medulla granulata *Seminum*, termino vitæ vegetantis." Then again the appendix to his *Philosophia Botanica*,[1] entitled "Metamorphosis vegetabilis" contains the following aphorism: "Principium florum et foliorum idem est". The full bearing of these passages can, however, only be appreciated when his theory of "Prolepsis," or anticipation, is understood. This theory is set out at length in two dissertations, both bearing the title "Prolepsis Plantarum,"[2] but the earlier one is the more important. The starting-point of the theory seems to have been the observation that when a tree or shrub is planted in a pot, it flowers and fruits annually; whereas if planted in open ground, it produces branches and leaves abundantly, but no fruit. Hence Linnæus infers that in the latter case leaves are produced in the place of the flowers in the former: and conversely, when such a plant is transferred from the open ground into a pot, flowers are produced in the place of leaves. He concludes that in the production of a flower there is an anticipation (*prolepsis*) of what would represent several years growth of vegetative shoots. The way in which he works out this idea, and forms his theory, will be made sufficiently clear by the following headings taken from the earlier dissertation: "Soboles præsentis anni *Folia* esse patet per se; Soboles insequentis anni *Bracteas* esse patet ex *Ornithogalis;* Soboles tertii anni est *Perianthium* (Calyx), quod patet ex *Luxuriantibus;* Soboles quarti anni *Petala*, quod patet ex *Proliferis;* Soboles quinti anni *Stamina* esse, patet ex *Plenis; Pistillum* staminibus exhaustis, esse ultimi anni folia a *Plenis* et *Carduis*." This idea, crude as it is, involves the assumption that the organs assigned to each successive year are equivalent; and Linnæus frequently designates them by the common term "*folium*".

It may seem strange that, having gone so far, Linnæus

[1] *Philosophia Botanica*, 1751; third edition, 1790, p. 301.

[2] "Prolepsis Plantarum" (Ullmark), 1760, in *Amoen. Acad.* vi.; "Prolepsis Plantarum" (Ferber) 1763, *ibid.*

should have failed to come to a full apprehension of the truth; but his failure may be easily accounted for. In the first place he was hampered by the prevalent fantastic theory of evolutionary development which had originated with Cæsalpini:[1] and in the second place his mind was doubtless so preoccupied with what has turned out to be the altogether unimportant theory of Prolepsis, that he was unable to perceive the great fundamental principle which lay exposed before him.

We pass now to Wolff. Though an animal physiologist and anatomist, Wolff turned his attention to plants, and with remarkable results, in the course of his investigations in support of his theory of Epigenesis by which he controverted the prevalent theory of evolutionary development. For the first time since Malpighi,[2] the minute structure and more especially the development of the organs of plants were carefully studied. As the chief result of his investigations, Wolff[3] made the important discovery that the "appendicular organs," as he termed the leaves, are developed at the growing-point, *punctum vegetationis* he called it, of the stem; and that this is true whether they be ordinary leaves, as he observed in the common Cabbage, or whether they be parts of the flower, as he observed in the Bean (*Vicia Faba*). Seeing, then, that all these appendicular organs have the same mode of origin, he concludes as follows:[4] "In the entire plant, whose parts we wonder at as being, at the first glance, so extraordinarily diverse, I finally perceive, after mature consideration, and recognise nothing beyond leaves and stem (for the root may be regarded as a stem). Consequently all parts of the plant, except the stem, are modified leaves."

With this quotation before us, no other conclusion can be drawn than that Wolff had anticipated Goethe's discovery in all essential points, though his exposition of it is

[1] *De Plantis*, 1583.

[2] Malpighi, *Anatomes Plantarum Idea*, 1675; as also other works in his *Opera omnia*, Lond. 1686; Lugd. Batav., 1687.

[3] *Theoria Generationis*, 1759; *Theorie von der Generation*, 1764.

[4] "De Formatione Intestinorum," *Nov. Comment. Acad. Petrop.*, xii., 1767.

less formally complete than that of Goethe. But this fact does not in the least detract from Goethe's merit; for, in spite of insinuations to the contrary, there is every reason to believe that Goethe had not seen the *Theoria Generationis* when he wrote his *Versuch*. When he did become acquainted with Wolff's work, he did not fail to recognise its importance, and spoke of its author as his "admirble precursor". It is extremely interesting to observe that these two men of distinguished ability, working independently, arrived at essentially the same conclusions by what I venture to regard as quite opposite methods. As to the nature of Wolff's method, there cannot be the slightest doubt. He approached the study of plants, it is true, for the purpose of establishing his theory of Epigenesis, but not with any preconceived ideas bearing upon Metamorphosis. He made his observations, and from them he drew his conclusions. But with Goethe the case seems to have been different. It is true that he made observations, but these observations would appear to have served rather to confirm a theory already formed in his mind than to suggest one. Whilst the method of Wolff was purely inductive, that of Goethe was essentially deductive. This is perhaps a hard saying, the more so as Goethe himself distinctly repudiates the *rôle* of an *à priori* philosopher.[1] "It is by no sudden and unexpected inspiration of genius," he writes, "but through long prosecuted studies that I have arrived at my results," a position which is even more definitely asserted in his first conversation with Schiller. Here is Lewes' account of it: "One day, in May 1794, they met, coming from a lecture given by Batsch at the Natural History Society in Jena; in talking over the matter, Goethe,

[1] In another passage, however, he seems to admit the deductive character of his work: "I saw that a whole life of talent and labour was requisite to enable any one to arrange the infinitely copious organic forms of a single kingdom of nature. Yet I felt that for me there must be another way, analogous to the rest of my habits. The appearance of the changes, round and round, of organic creatures had taken strong hold on my mind. Imagination and Nature appeared to me to vie with each other which could go on most boldly and most consistently." (*Zur Morphologie*, 1817, i., p. 30).

with pleased surprise, heard Schiller criticise the fragmentary method which teachers of science uniformly adopted. When they arrived at Schiller's house, Goethe went in with him, expounding the theory of Metamorphosis with great warmth. Taking up a pen he made a rapid sketch of the typical plant. Schiller listened with great attention, seizing each point clearly and rapidly, but shaking his head at last, and saying: 'This is not an observation, it is an Idea'. Goethe adds: 'My surprise was painful, for these words clearly indicated the point which separated us. The opinions he had expressed in his essay on *Anmuth und Würde* recurred to me, and my old repulsion was nearly revived. But I mastered myself, and answered that I was delighted to find I had ideas without knowing it, and to be able to contemplate them with my own eyes.' There can be no question of Schiller having been in the right. . . . The typical plant, Goethe knew very well, was not to be found in nature; but he thought it was *revealed* in plants. Because he arrived at the belief in a type through direct observation and comparison, and not through *à priori* deduction, he maintained that this type was a perception (*Anschauung*), not an idea." In another place (p. 345) Lewes expresses the same view in his appreciation of Goethe as a man of science: "Goethe was a thinker in science, a manipulator of scientific ideas. He was not one of those laborious and meritorious workers who with microscope and scalpel painfully collect the materials from which Science emerges. He worked, too, in his way, and everywhere sought in the order of nature for verification of the ideas which he had conceived *à priori*. Do not, however, mistake him for a metaphysician. He was a positive thinker on the *à priori* method; a method vicious only when the seeker rests contented with his own assumptions, or seeks only a partial hasty confrontation with facts—which Bacon calls *notiones temerè a rebus abstractas;* a method eminently philosophic when it merely *goes before* the facts, anticipating what will be the tardy conclusions of experience."

Moreover, it may be pointed out that the evidence upon which Goethe relied was insufficient to warrant the generali-

sation at which he arrived. For what is this evidence? It consists, in the first place, of a careful comparative study of the appendicular organs, noting the various forms which they assume in different parts of any one plant as well as in different plants, with special reference to those cases, to be found here and there, where transitional forms occur. But external resemblances are by no means conclusive; it would be easy to cite numerous examples of close external similarity between organs as distinct as stems and roots, stems and leaves, and roots and leaves; in fact several examples of the kind are now before you. Then again he relies upon monstrosities, such, for example, as stamens which have assumed the character of petals, as sepals that of ordinary green leaves. But teratological evidence is quite inconclusive, though it is suggestive; for when any part deviates in form from the normal, it does not follow that the form which it assumes is a primitive form indicative of its essential nature; there is no conclusive reason for regarding such a case as one of reversion. What was lacking in Goethe's observations was largely supplied by Wolff's elaborate developmental studies; for the study of development is the only sure means of determining the nature, and thus also of establishing the homologies, of the parts of any living organism.

In reviewing the doctrine of metamorphosis in plants as it was left by Wolff and by Goethe, we cannot fail to perceive that, great as was the advance they had made, it was far from complete. In the first place, its application was confined to one set of organs, the appendicular organs, neglecting others of equal importance, such as the stem and the root. And even with regard to the appendicular organs much was left to be desired. They tell us that all these organs are "leaves," and that the character common to them is their mode of development from the growing-point of the stem. They further tell us that the organs of the flower are "modified leaves". But the question remains, of what leaf or leaves are they the modifications? To this question two alternative answers have been offered. The one which we may distinguish as the concrete,

suggests a material metamorphosis of one kind of leaf into the other; for instance, on this view a sepal might be regarded as a metamorphosed foliage-leaf, a petal as a metamorphosed sepal, a stamen as a metamorphosed petal, and so on. The result of this process would be that the foliage-leaf would come to be regarded as the primitive typical form of leaf to which all the others might be traced back; a view which became discredited in consequence of the extravagant application of it by the school of "natural philosophers" which Goethe's *Versuch* may be said to have called into existence; whose members, following in the footsteps of Goethe rather than of Wolff, but without Goethe's genius, speculated instead of observing. Then there is the other answer, the abstract or transcendental answer, that the various forms of leaves are modifications not of any existing form, but of some imaginary typical leaf. It would appear that this latter answer is the one which Goethe offered, though he is not altogether clear on the point. Having, in § 119 of the *Versuch*, pointed out that all the various organs of the flowering plant may be referred to one, the leaf, he proceeds in § 120 as follows: "It is obvious that we ought to have a general term by which to designate the organ which is metamorphosed into so many forms; for the present we must accustom ourselves to realise that these phenomena take place backwards and forwards. For we can as well say a stamen is a contracted petal as we may say of the petal that it is an expanded stamen; or that a sepal is contracted foliage-leaf, as that a foliage-leaf is an expanded sepal." Evidently, whilst he had arrived at the extension of the term "leaf," he was unable to formulate the intension of the term. What he sought was the morphological concept of the leaf; and the reason why he failed to form it was that the morphological Botany of his time was too superficial and too physiological to admit of such conception.

But now I must give you some idea of what I mean by a morphological conception of the plant, or of part of it. You may perhaps have noticed that so far I have uniformly spoken of the various parts of plants as "organs," and I

have done so because this was the terminology of the period in the history of Botany which we are considering. This mode of regarding them had come down from Aristotle,[1] who defines "the parts of plants as organs,[2] though quite simple ones; thus the leaf is the covering of the pericarp, the pericarp of the fruit; the roots are the analogues of a mouth, for both absorb nourishment": and again,[3] "the nature of plants being stable, there are not many kinds of heteromerous parts; in the few kinds of work, there is use for but few organs; wherefore they are to be considered with regard to their form". That is, all the parts of plants were considered primarily from the physiological point of view of their function; analogies had been drawn between them, but not homologies. Some indication of a morphological conception of them—that is, of their nature apart from their function—is to be found in the writings of Theophrastus of Eresus;[4] but no real progress was made in this direction until late in the seventeenth century, when Joachim Jung, a philosopher, sometime Professor of Logic, Physics and Metaphysics, and ultimately Rector of the Johanneum in Hamburg, wrote his remarkable botanical treatises,[5] in

[1] Aristotle's work on plants (θεωρία περὶ φυτῶν) is lost, though a treatise, *De Plantis*, is generally included in editions of his works, of which treatise Whewell says (*Hist. Induct. Sciences*, iii.) that it is "an imposture of the middle ages, full of errors and absurdities": but his extant writings contain numerous references to plants: all these passages have been collected by Wimmer under the title "Phytologiæ Aristotelicæ Fragmenta" (1838), of which Meyer gives a translation in the first volume of his *Geschichte der Botanik*, 1854.

[2] *De Anima*, ii., cap. i.

[3] *De Partibus Animalium*, ii., cap. x.

[4] *Historia Plantarum*, lib. i., cap. ii.

[5] They were not published until after his death, when they were issued by his pupils, Fogel and Vagetius. The most important is the *Isagoge Phytoscopica*, 1678. Some of his definitions are of sufficient interest to warrant quotation; I quote from Albrecht's edition of the collected works (*Opuscula Botanico-Physica*, Coburg, 1747).

"*Planta* proprie dicta duabus constat partibus, radice et superficie sive parte superna."

"*Radix* est pars inferior quæ intra corpus solidius quod plantæ sedem præbet, abdita, et alimento attrahendo destinata est. *Pars superna* est,

which he reveals striking morphological insight. Whilst his definitions may be criticised as lacking in precision, it must be admitted that he had grasped the fundamental ideas of morphology. Analysing the plant-body into root and shoot, and the shoot into stem and leaf, he defines these parts, not according to their function, but according to their relative position and their form; that is, he defines them not as "organs," but as "members" of the body.[1]

As in the case of Wolff and of Goethe, so also in that of Jung, the publication of new and epoch-making views produced no effect upon the contemporary science, beyond the fact that Jung's work was incorporated by Ray in his *Historia Plantarum* (1686); nor, in Jung's case, was the neglect of his contemporaries atoned for by the recognition of posterity.[2] Certainly there is no trace of his influence

quæ supra sedem plantæ in liquido corpore (aëre vel aqua) exstat, et speciei eminus propagandæ præcipue inservit."

"*Limes* communis, in quo duæ istæ partes cohærent *Fundus plantæ* dicitur, Græcis πυθμήν."

"Pars superna, aut pars partis supernæ est vel *Caulis*, vel *Folium*, vel *Flos*, vel *Fructus*, vel minus secundaria aliqua pars, v.g. *Villus* aut *Spina*."

"*Caulis* est pars superna, in altitudinem ita exporrecta, ut anteriora a posterioribus, vel dextra sinistris non differant."

"*Folium* est, quod a sede, cui adhæret, ita in altitudinem, sive longitudinem, et latitudinem extenditur, ut tertiæ dimensionis termini inter se differant, h. e. superficies folii interna ab externa."

"Superficies folii interna, quæ et *superior*, item *supina* dicitur, est quæ caulem respicit, ideoque vel cavitatis aliquid obtinet, vel minus convexa est, quam altera *exterior* sive *inferior*, sive *prona* superficies."

"*Perfecta planta* vel caulem a folio distinctum gerit vel confusum cum eo. Quæ confusum gerit, ea est, quæ folium e folio producit, atque, ita ex meris foliis caulem quasi quendam ramosum componit, qui eo *discrepat a natura* caulis, quod partes prismatis figuram non referunt, sed crassitiem latitudine minorem habent *h.e. in latitudinem expansæ* sunt, ideoque etiam *folia* dicuntur. Rursus in eo *caulis indolem exprimit* hoc folium, quia *se propagat*, quod folio proprie dicto non competit. Differt etiam id, quod folium dicitur in his plantis a folio proprie dicto, quod *superficies* latiores, figura non diversas, h.e. *interiorem ac exteriore non differentem* obtinet."

[1] See Hofmeister, *Allgemeine Morphologie*, 1868, p. 409, footnote 2; Hanstein, *Bot. Abhandlungen*, i., 1870, p. 92.

[2] Albrecht, in his preface to the collected botanical works of Jung, says on this point: "Mirati sumus sæpius, qui factum sit, ut in historicis

in the works of Linnæus, any more than in those of Wolff and of Goethe. But Wolff's own work contributed materially to the advance of morphology. As already pointed out, he established a definite contrast between the stem as the axial member of the body, and the leaves as the appendicular members. Whilst it is true that he regarded the root as of the same nature as the stem, it is clear that he did not do so in consequence of any confusion of ideas; what he meant to convey was that they are to this extent of the same nature, that they are both axial, as distinguished from appendicular, members. Far from confusing them, he was the first to point out one of their fundamental morphological differences, namely, that the stem bears leaves whilst the root never does so; and further, he discovered the endogenous mode of development which is characteristic of the root.

If we now compare the definitions of the chief members of the plant given by Aristotle, Linnæus and Wolff, the essential difference between the physiological and the morphological points of view stand out in striking contrast. Taking the root, for example, we find that Aristotle regards it as the organ for the absorption of nourishment and that Linnæus describes it in almost identical terms: "Radix descendens, aquosa sorbens, nutriens" (*Syst. Nat.*); while Wolff sees in it the part of the plant which does not bear leaves. At the same time it must be clearly apprehended that these two points of view are not mutually exclusive: for whilst the root is distinguished morphologically as the member of the plant-body which bears neither leaves nor flowers, it may be regarded physiologically, and with equal justice, as an absorbent organ. It is not too much

de fatis et incrementis studii botanici scriptis, multo minus vero in Isagogicis seu Institutionibus nulla fere, vel saltem rarior Jungianis meritis, quæ sane in hac re magna sunt, digna legatur commemoratio, Quin ipse solertissimus et in perfectiorem rei botanicæ emendationem natus Linnæus Doxoscopiarum Jungianarum ne meminit quidem, licet vir sit in bibliothecis botanicis versatissimus, atque Isagoges fecerit justam mentionem. Causam sane aliam suspicari non licet, nisi quod ad oculos tanti viri illæ non pervenerint, alias enim integerrimum virum Jungium nostrum, nisi in critica botanica socium sibi adjunxisse, certe tamen inter Philosophos æque primum locum illi assignaturum fuisse non dubitamus."

to say that one of the main obstacles to the advance of morphological Botany has been the failure to recognise the distinctness of these two conceptions; the confusion of what any part of the plant *is* with what it *does*.

But I am anticipating somewhat. Before I can properly develop the foregoing ideas, we must trace the onward progress of morphology since the time of Goethe.

As I have already indicated, the period immediately succeeding the publication of Goethe's *Versuch*, up to nearly the middle of the present century, was one fruitful in little else than wild theorising; it was the period of the so-called "natural philosophers". This fortunately culminated in a reaction to investigation and induction. On a sudden, as it were, a band of men arose, of brilliant ability and indefatigable industry, whose great achievements have revolutionised not only the department of morphology, but the other branches of Botany as well; I need only mention the names of Schleiden, von Mohl, Naegeli, Hofmeister, Robert Brown, Irmisch, Hanstein, Alex. Braun. It would take me too long to give you anything like a detailed account of their researches; to do so would be to write at length one of the most important and striking chapters in the history of Botany. All that I can hope to do is to lay before you some of the results of this remarkable renascence of our science.

I need hardly say that the morphological distinction of the three chief members of the plant as established by Wolff, root, stem, leaf, has been fully confirmed, and has been crystallised in the abstract terms *caulome* and *phyllome*[1] for stem and leaf; the similar terms *trichome* and *thallome* being added to designate, the former the epidermal members of the body (hairs, etc.), the latter the undifferentiated body, characteristic of the lower plants, which presents no distinction of the three primary members. Another important result has been the extension of the idea of meta-

[1] Naegeli und Schwendener, *Das Mikroskop*, 1867. The correlative term *rhizome* for the root was not introduced, probably because the word was already in use in Botany, being applied to a creeping underground stem.

morphosis, which had hitherto been confined to the leaves, to the other members. For instance, we now know that a tuber, such as that of a potato, or of a Jerusalem artichoke (*Helianthus tuberosus*), is not a root, though it is subterranean, but is a metamorphosed branch; for if the young shoots at the lower part of the potato-stem be not covered up with earth, as is done in the ordinary course of cultivation, they develop into green leafy branches; whereas, when buried in the soil, they develop into potatoes. Again, we now know that such things as turnips and carrots are really metamorphosed roots.

But with this extension of the scope of metamorphosis, important knowledge has been gained as to its limits. It may be now laid down as a general law that metamorphosis can only take place within each category of members, and not from the one to the other;[1] that, for instance, a leaf cannot be metamorphosed into a stem, or a stem into a leaf, or either of them into a root. At the same time it frequently happens that one member may discharge the functions of another, thus becoming analogous to it; but in doing so it does not change its morphological nature and become homologous with the member whose work it has taken over. For instance, I would draw your attention to the Cactus (*Opuntia*) on the table before you. The flattened lobes of which its shoot consist somewhat resemble leaves in form, and actually perform the functions of leaves, yet morphologically they are not leaves, but leaf-like segments of the stem, the plant being leafless. The same thing is even more strikingly shown by this branch of the Butcher's Broom (*Ruscus*) where the stem, which is of normal form, bears what look like leaves, but are really leaf-like branches (*phylloclades*). Again, one part of a member may assume the form and discharge the functions of another. Here is a branch of an Australian Acacia in which the stems bear organs resembling leaf-blades; they are, however, not leaf-

[1] It must, however, be admitted that exceptions do occur. Thus the conversion of the root into a shoot has been observed in *Neottia Nidus-avis* (see Hofmeister, *Allgemeine Morphologie*, 1868, p. 428), and in *Anthurium longifolium* (see Goebel, in *Bot. Zeitg.*, 1878).

blades but leaf-stalks (*petioles*), which simulate the leaf-blade and discharge its functions (*phyllodes*); in this plant the leaf-blade, which is usually the important organ of nutrition, is absent. Rootless plants offer good examples of the assumption of a root-like form and function either by underground stems (*Psilotum*, saprophytic orchids such as *Neottia*, *Corallorhiza*, *Epipogum*), or by leaves (*Salvinia*). It would be easy but useless to multiply examples.

We may now proceed to inquire what is the attitude of modern morphology towards the doctrine of metamorphosis. You will remember that I have mentioned the two possible answers, the one concrete, the other abstract, to the important question, what is it that undergoes metamorphosis? Are we to regard all the various forms of leaves, for instance, as modifications of an ideal "type"—of an "*Urblatt*"—or of some actually existing form? Let us see what is the verdict brought in by actual research.

The point is one which has been much discussed. It is not unnatural that some of the earlier writers, whilst still under the influences of Goethe and his immediate followers, should have accepted the abstract view. Thus we find Alex. Braun writing:[1] "This ideal ladder which Goethe perceived in the metamorphosis of plants, is a speaking testimony of the profound conception of it which he had formed; for that which leads the formative process of the plant from one rung to the next, which connects the rungs into a ladder, which reveals each rung, though distinct from its predecessors, as a product of their modification, cannot be other than inward and ideal". Moreover in a footnote he says further: "That Goethe was not altogether free from the erroneous notion that one organ of a plant might be actually transformed into another, for instance, stamens into petals, or pistils into leaves, is evident from the very first paragraph of his introduction". The same position has also been taken up by some later writers. Thus Schmitz[2] says: "After all, stamens and foliage-leaves

[1] "A. Braun, Verjüngung in der Natur," 1851: p. 61 of the English translation, *Botanical and Physiological Memoirs, Ray Society*, 1853.

[2] Schmitz, "Die Bluthen-Entwickelung der Piperaceen," in Hanstein's *Bot. Abhandl.*, ii., 1872, p. 32, footnote.

are merely phyllomes which are differentiated, having assumed different forms in relation to their diverse functions: but stamens are by no means metamorphosed foliage-leaves. Stamens and foliage-leaves are only equivalent, as regards their relation to the entire shoot, to the axis which bears them." And again Hanstein,[1] in one of his last memoirs, writes: "Further, the fact that all these various forms of leaves succeed each other from below upwards on the shoot, and are, at the same time, frequently connected by intermediate forms so that the primitive identity of their morphological nature is more clearly brought to light, reveals them as modifications of one and the same organic type which successively transforms itself into each of these organs. This process, which is rather theoretical than actual, is what is termed the Metamorphosis of the leaf."

On the other hand Schleiden,[2] faithful to the traditions of Wolff, stoutly maintains the view of material metamorphosis. Beginning with the blunt statement that "Science, to her great detriment, received this thought (metamorphosis) not from Wolff but from Goethe, a thought which might have proved so fruitful, but which, owing to the manner in which Goethe introduced it, has been of relatively so little use," Schleiden goes on to give his conception of metamorphosis as "the fact that the plant possesses but few essentially different organs, and that all the others differ only dynamically from these fundamental organs in that there is inherent in them a tendency to undergo a definite and characteristic development and modification; a tendency which is, however, not so absolute but that it can be overcome under special circumstances, and then the normal form of the organ again becomes apparent". The language of this confession of faith is somewhat involved, but its purport is unmistakable.

There can no longer be any doubt that the weight of

[1] Hanstein, "Beitr. zur allgemeinen Morphologie der Pflanzen," *Bot. Abhandl.*, iv., 1882, p. 30.

[2] Schleiden, "Einige Blicke auf die Entwickelungs-geschichte des vegetabilischen Organismus bei den Phanerogamen;" in his *Beiträge zur Botanik*, 1844, p. 86.

accumulated evidence is distinctly in favour of the material view of metamorphosis; that is, of a genetic relation between the various forms of any one member. To take some simple illustrative cases: it is clear, for instance, that a potato-tuber is produced by the actual metamorphosis of an ordinary branch; nor is it less clear that a turnip is the product of the actual metamorphosis of an ordinary root. The matter becomes more complicated, when we turn to the leaves, on account of the great variety of form which these members present, and calls therefore for more elaborate treatment. For whereas in the cases of shoot and root just mentioned, only a single metamorphosis takes place; in the case of the leaves, on the contrary, we have the question of successive metamorphoses to consider, the transformation of one metamorphosed leaf into another.

In order to deal adequately with the metamorphosis of leaves, it will be necessary for me to inflict upon you a brief statement of the different classes or categories of leaves to be met with on the plant, based on the classification proposed by Alex. Braun[1] and by Hanstein.[2] They distinguish:—

Belonging to the Vegetative Region.

1. The *Cotyledons*, or seed-leaves, the first leaves of the embryo plant.

2. The *Cataphylls*, or scale-leaves, such as form the covering of buds.

3. The *Euphylls*, the ordinary green foliage-leaves.

Belonging to the Reproductive Region.

4. The *Hypsophylls*, including:—

   (*a*) The *bracts*, which are intermediate between the foliage-leaves and the floral leaves; (*b*) the *sepals*, forming the calyx; (*c*) the *petals*, forming the corolla.

[1] Alex. Braun, "Verjüngung," Eng. Trans., p. 62.

[2] Hanstein, *Bot. Abhandlungen*, iv., 1882, p. 28.

5. The *Sporophylls*,[1] the essential reproductive organs, including :—

(*a*) The *stamens*, forming the androœcium; (*b*) the *carpels*, forming the gynæceum or pistil; in Flowering-plants.

But this enumeration by no means exhausts the possibilities of leaves, for these various categories may themselves exhibit polymorphy to a remarkable degree. For instance, some of the cataphyllary leaves of the onion, which is nothing but a subterranean bud, become enlarged to serve as depositories of nutriment; again, the leaves belonging to the category of euphylls present, besides the endless varieties of their typical form, a wide range of modification, assuming forms so diverse as that of spines in the Barberry, of tendrils in the Pea, of tentacular leaves in the Sun-dew, of pitchers in *Nepenthes* and *Sarracenia*.

What now is the evidence to prove material metamorphosis in the case of leaves? When we have to deal with simple metamorphosis, with metamorphosis that is taking place within the limits of any one category of leaves, the evidence is as convincing as that which I have adduced with regard to stems and roots. For instance, no one has ever suggested that the spiny leaf of a Barberry, or the pitchered leaf of *Nepenthes*, is anything but a modification of an ordinary foliage-leaf; or that the nectary of the Monks-hood is anything but a modified petal; or that the petaloid investment of the inflorescence of the Trumpet-Lily (*Richardia*) is anything but a bract. The most superficial observation would suffice to disprove any contrary assertion. But when we go on to consider the possibility of a genetic relation existing between leaves of different categories, we have a more complex problem to solve. However, some satisfactory evidence on this point is forthcoming; based to some extent on the actual observation of development, but mainly on the occurrence of intermediate forms. The consideration of the cataphylls affords a good

[1] We owe this term to Schleiden, *Grundzüge der wissenschaftlichen Botanik*, English Edition, 1849, pp. 194, 346.

illustration of the developmental evidence. Prof. Goebel,[1] to whom we owe most of our knowledge on this subject, has shown that, in many cases, the scales protecting the leaf-buds are really foliage-leaves which have undergone modification which consists either in the abortion of the leaf-blade, or in a change of form of the entire leaf. He has ascertained this, not only by the study of development, but also experimentally in a most ingenious way. He has found, namely, that if the leaf-buds formed in any one year to expand in the following year, are forced to expand in the year of their formation,—which may be accomplished either by cutting off the growing end of the branch bearing the buds, or by removing all its leaves—the buds form no bud-scales, but the structures which would have formed bud-scales in the normal course now develop into foliage-leaves instead. With regard to the bracts, the matter is comparatively simple; in many cases they are quite indistinguishable from the foliage-leaves, and in others their relation to them can be traced by a series of intermediate forms. The same kind of evidence applies also to the sepals. In many cases they cannot be distinguished from the bracts; and it must be further borne in mind that, inasmuch as they stand in the same relation to the flower-bud as do the bud-scales to the leaf-bud, there is a certain connection to be traced between them. Coming now to the petals, we find that in many cases (as in the Lily) they cannot be distinguished from the sepals; moreover their peculiar attributes of colour and form may be exhibited, not only by the closely related sepals, but by the more remotely related bracts (*Bougainvillea Pointsettia*). Finally, in the case of the sporophylls, we have to rely upon the manifestation by them of phylloid characters so frequent in the case of monstrous and of double flowers, as well as upon the comparatively few cases in which the petaloid form is the normal (stamens of *Canna* and its allies; petaloid style of *Iris*).

But if this evidence suffice to prove the existence of material metamorphosis in the category of leaves, the

[1] Goebel, *Vergleichende Entwickelungsgeschichte*, p. 248.

questions still remain, is there some primary form of leaf of which all the others may be regarded as modifications? and, if so, what is that form? The first of these questions must, as it seems to me, be answered in the affirmative; for material metamorphosis necessarily involves such an assumption. We have traced a series of forms of leaves, differing the one from the other by the characters which they have assumed as the result of adaptation to their several functions: the cataphylls to protect the buds; the foliage-leaves to discharge certain nutritive functions; the sepals to protect the flower; the petals to attract the visits of insects; the stamens and carpels to bear the reproductive cells; and this series must have had a definite starting-point. In endeavouring to determine this starting-point, to discover this primitive form, it is evident that two alternatives are open to us; the series has two ends which we may distinguish as the higher and the lower, and it is obviously possible to take one or the other of these ends as the starting-point, to regard either the sporophyll or the foliage-leaf as the primitive form. Goethe long ago was struck by the fact that both *ascending* (from the foliage-leaf upwards) and *descending* (from the sporophyll downwards) metamorphoses are exhibited by plants. Which of these modes is the course actually followed in the differentiation of the organs of the plant?

As a matter of fact, both these possible views have their supporters; and the point may be regarded as still open to discussion. There can be no doubt that those who first introduced the idea of material metamorphosis, basing themselves upon the ascertained fact that the leaves are developed in acropetal succession at the growing-point of the stem—so that the youngest leaves are nearest to, the oldest most remote from it—adopted the theory of ascending metamorphosis, regarding the foliage-leaf as the primitive form of which all the others are modifications as the result of special adaptation to their various functions. In the present day the strongest supporter of this view is Goebel, whose position may be gathered from the following quotation: "Herein lies the morphological equivalence of the

leaf-organs, that the plant, as a matter of fact, gives origin to leaves of one kind only, namely the foliage-leaves, the ultimate form of which is modified in various ways by conditions which arise in the course of their subsequent development". Whilst Goebel agrees with his predecessors in regarding the foliage-leaf as the material basis of metamorphosis, it appears that there is a difference between them as to the mode of the process: according to the older view the more highly modified organs are the products of *mediate* metamorphosis of the foliage-leaf; the stamen, for instance, being traceable to it through the petals and sepals: Goebel, on the contrary, asserts that metamorphosis is *immediate*, regarding all other forms of leaves as being directly traceable to the foliage-leaf, though they may be sometimes transformed into one another.

The view that the foliage-leaf is the primitive leaf-member, and that the floral leaves are its derivatives, is then based upon the fact that, as a rule, the vegetative precede the reproductive organs in ontogenesis. The opposite view that the most highly specialised floral leaf, the sporophyll, is the primitive phyllome, is based upon the fact that, phylogenetically, the reproductive precede the vegetative leaves. The full discussion of the latter view would involve an amount of technical detail which would be altogether out of place in such a lecture as this. I must content myself with briefly explaining that in such lowly organised plants as the Mosses (Bryophyta), the sporophyte, that is the form in the life-history which correspond to the Flowering-plant, consists, in the simplest case, of little more than a mass of reproductive cells; and even in the more complex Mosses, this form never presents any differentiation of vegetative organs. We find, then, that in such primitive plants as the Mosses, the reproductive is the predominant function. Turning now to the Fern-like plants (Pteridophyta), the plants which come next above the Mosses, we find that they have well-developed leaves; and further, that in the more primitive forms the leaves are distinctly differentiated into sporophylls and foliage-leaves; whereas in what may be regarded as the secondary forms, this distinction is either

slight or altogether absent. Prof. Bower,[1] who has recently urged with great force the claims of the sporophyll to be regarded as the primitive leaf, sees in the differentiation of leaves in the Ferns, first of all an increase of the reproductive, spore-bearing area, as compared with the Mosses, by the development of sporophylls; and then the further evolution of the nutritive functions by the differentiation of some of the sporophylls as foliage-leaves. The relative positions of Goebel and of Bower may be summed up in a single sentence: whereas Goebel regards a sporophyll as a foliage-leaf which has become metamorphosed in consequence of the development of spores from its substance, Bower regards a foliage-leaf as a sporophyll which has lost its primitive form in consequence of sterilisation. The Fern-like plants are the main battle-ground upon which this question is being contested. When the issue is decided there, it will also have been decided for the Flowering-plants (Phanerogamia); for whatever is true of the relation between the various forms of leaves in the Fern-like plants, must also be true with regard to their descendants the Flowering-plants. It is, therefore, not necessary to attempt a discussion of the matter as regards Flowering-plants: I will merely mention the significant fact that the flowers of the earliest Flowering-plants (Gymnosperms) consist entirely of sporophylls, being altogether destitute of petals and sepals, whence the primitive nature of the sporophyll may be fairly inferred.[2]

[1] Bower, "A Theory of the Strobilus in Archegoniate Plants," *Annals of Botany*, viii., p. 343, 1894: "Studies in the Morphology of Spore-producing Members—Equisetineæ and Lycopodineæ," *Phil. Trans. Roy. Soc.*, vol. 185 B, 1894, p. 473. Some interesting facts supporting Prof. Bower's view have recently been published by Atkinson, *The Transformation of Sporophyllary to Vegetative Organs*, Boston, 1896. He has found in certain Ferns (*Onoclea sensibilis* and *Onoclea Struthiopteris*) which have clearly differentiated sporophylls and foliage-leaves, that if the young foliage-leaves be destroyed early in the season, the sporophylls (which develop later) do not present their characteristic form, but more or less resemble the foliage-leaves and are partly or completely sterile. These observations show that when nutritive functions are, as it were, forced upon sporophylls, they assume the habit of foliage-leaves.

[2] This point is well brought out by Mr. Grant Allen in his *Colours of*

We see that, in spite of the differences of opinion as to the precise mode in which it takes place, modern Botany has established the doctrine of material metamorphosis upon a sound basis of observed fact; and in arriving at this result I have accomplished one of the chief objects of this lecture. I will, however, venture to trespass on your patience for a few minutes longer to briefly consider in conclusion some of the theories which have been propounded for the purpose of explaining the occurrence of metamorphosis.

It is easy to account for the various metamorphoses of the members of the plant on the metaphysical principle of final causes, by simply asserting that any given metamorphosis is to the advantage of the plant—for instance, that the metamorphosis which results in the production of petals is to be attributed to the advantage gained by the plant in the cross-pollination by means of insects which the brightly coloured petals ensure—and, no doubt, as far as it goes, this explanation is sound. But I desire to lead you farther than this, and to inquire on the physical method whether or not some efficient cause may be found; and, if so, what the nature of the efficient cause or causes may be.

Some light will be thrown on this difficult matter by the consideration of metamorphoses, which we may distinguish as traumatic, caused by the attacks of insects and other parasites. It is known that chloranthy, a condition in which all or the great majority of the floral organs assume the form and colour of foliage-leaves, is frequently induced as the result of puncture by insects or by the growth of fungi in the tissues[1]. Here then is a case of metamorphosis which can be clearly traced to an efficient cause; but we must endeavour to penetrate farther into the matter and ascertain as far as may be the mode in which the attacks of these parasites bring about so striking a change.

Flowers, "Nature Series," 1882. His point of view is, however, somewhat different from that stated above, in that he assumes that "The starting-point consists of a plant having three kinds of organs, true foliage-leaves, staminal leaves and carpellary leaves" (p. 14).

[1] Masters, *Vegetable Teratology*, 1869, 279 (Ray Society).

Turning now to those cases of metamorphosis which cannot be traced to injury, we find ourselves at once confronted with the great biological problem of the antagonistic relation existing between growth and reproduction. The fundamental observation illustrating this is that recorded by Linnæus as the basis of his theory of Prolepsis, to which I have already drawn attention—the observation, namely, that when a tree is abundantly supplied with nourishment it develops leafy shoots, whereas when transplanted to poor soil, or to the narrow limits of a pot, it develops flowers. This idea of the relation between deficient nourishment and flowering—that is, reproduction—also occurs in Wolff's writings, for he refers flowering to a *vegetatio languescens et evanescens*. With Goethe the idea takes a somewhat different form. He assumes that the foliage-leaves on the lower parts of the plant are products of coarser nutritive sap which, as it ascends, undergoes refining by filtration and thus becomes suited for the development of the floral organs.

On comparing the views of Linnæus and of Goethe, we observe that they differ in that the former refers metamorphosis of the leaves entirely to external, the latter entirely to internal causes. We will briefly consider this point. Whilst there is no doubt that external conditions may induce, there is evidence to show that they do not originate, metamorphosis. To return to an example already cited: it is true that the lower shoots of the potato-plant will not develop into tubers unless they are covered up with earth; but this treatment does not confer upon them the capacity of developing into tubers, for the shoots of other plants treated in the same way do not so develop. Again, in Linnæus' observation given above, whilst it is true that a certain change in the environment of a tree may hasten its flowering, this change does not confer upon the tree the power of flowering; that it possesses already, and would eventually manifest it even though left to grow in a rich and unlimited soil. And, finally, in the case lately quoted of chloranthy resulting from injury, the puncture of the insect would lead to no such change were not the capacity for it already inherent in the plant.

Whilst then we admit that external conditions and agencies may be the *vera causa* of any given metamorphosis, we cannot fail to perceive that they constitute but the proximate cause, whilst the ultimate cause resides in the organisation of the plant itself. This has long been recognised, and a plastic force or *vis formativa*,[1] whereby it develops the characteristic form of its organs, has been attributed to the living organism. I need hardly point out, however, that such an assumption as this is no explanation of the phenomena; it is merely a statement, in abstract form, of the facts of development. It is obviously true, but it is no more than a truism beyond which we must pass if we would attain to a knowledge of the physical conditions which are the ultimate causes of the phenomena. The only attempt in this direction with which I am acquainted is that of von Sachs[2] —whose recent loss all Botanists have to deplore—who has suggested that the development of the different kinds of organs is the result of the elaboration of certain specific plastic materials in the plant, a view to which Goebel also adheres to in the main.[3] But this suggestion has not been sufficiently worked out to warrant its acceptance. And even were it proved to be true, the *vis formativa* would not have been explained away, for the questions would still remain: why and how does any particular plant produce just those substances which are necessary for the development of its own peculiar forms of leaf, flower and fruit; and how is the appropriate distribution of these substances in the plant effected at just the right time and in proportionate quantity? We must admit that Science has not yet lifted the veil which enshrouds this last mystery of metamorphosis.

I have now concluded my task. I have laid before you to the best of my ability the history of the origin and growth of the doctrine of metamorphosis. With regard to

[1] Blumenbach's *Nisus formativus* (Ueb. den Bildungstrieb und das Zeugungsgeschäfte, 1781): see Vöchting, Ueb. Organbildung im Pflanzenreich, i., 1878.

[2] Von Sachs, "Stoff und Form der Pflanzenorgane," *Arb. d. bot. Inst. in Würzburg*, ii., 1882.

[3] Goebel, *Vergleichende Entwickelungsgeschichte*, p. 113.

the present position of the doctrine, we have seen that the result of the vast amount of research which has been carried on since it was first promulgated, has but served to establish it upon a broader and sounder basis. Moreover it has been pointed out that the concept of a material metamorphosis—that is of a genetic relation between the different forms of the same member—is the one which has the weight of evidence in its favour; though opinions are still divided, in the case of the leaf, as to whether the primitive form was a sporophyll or a foliage-leaf. In using the expression "material metamorphosis," it is not implied that every metamorphosed organ once actually possessed the form of its primitive member, assuming it as a necessary phase in its development; on the contrary, the idea involved is that, whilst all the embryonic organ-rudiments are potentially capable of developing into their appropriate primitive form, many of them do actually develop, under normal conditions, more or less directly into secondary, metamorphosed forms. It will also have been gathered that metamorphosis has a deep physiological significance, inasmuch as it is by this means that the few primitive members of the plant become multifariously adapted as organs for the performance of a great variety of functions; and herein the leaf is prominent as the most plastic member of the body. In fact, a higher degree of metamorphosis is the external expression of a more complete division of physiological labour.

But if the store of knowledge on the subject has amazingly increased since the time of Wolff and of Goethe, it is still far from complete. This is a confession which has always to be made when stock is taken of what is known on any scientific subject; for in no branch of Science is finality even in sight, and it is doubtless this that lends charm to the pursuit. In this subject we have still to discover the mysterious law of which Goethe speaks in the lines:—

> Alle Gestalten sind ähnlich und keine gleichet der andern;
> Und so deutet das Chor auf ein geheimes Gesetz,
> Auf ein heiliges Räthsel.

S. H. VINES.

THE BACILLUS OF PLAGUE.

AMONG the services rendered by bacteriology to medicine, not the least is the discovery of the micro-organisms which are the cause of two diseases that have lately reappeared in epidemic form. Both influenza and the plague, diseases well known to our forefathers, have been proved to be of bacterial origin, and as the result a mass of conflicting and contradictory evidence on the causation of these diseases has been cleared away.

Although the recent spread of the plague in China and India has drawn the attention of most civilised governments to the question of how the possible introduction of this disease into Europe may be prevented, it is worth noting that during the last forty years from 1855-95 the plague has appeared more than sixty times as large or small epidemics. Mahé (1) has pointed out that most of these outbreaks have occurred in Southern China, but on eighteen separate occasions it has been seen in Persia, twenty-six times in Turkey and five in Turkestan. One epidemic is recorded in South-east Russia, while in India, especially in the districts of Kumaon and Garwhal, the plague has appeared at least six or seven times. A study of the outbreaks in Marseilles in 1720, in Moscow in 1770, in Cairo and Smyrna in 1834, together with the one so well described by Russell in Aleppo during 1760-63, was until four years ago the chief source of our knowledge of the causation, symptoms and progress of the plague; and although no material advance has been made in the clinical history of the disease, it is interesting to recall some ideas which were current as to its causation. "During the continuance of the epidemic effect," writes an author fifty years ago (2), "a principle is given off from the body which, if very concentrated and pent up in confined and unwholesome situations, may generate the disease, so that, though not originally contagious, it may in this way by accumulation of animal miasms be contagious; and when the disease is communi-

cated from person to person it is by the inhaling the pestiferous breath or exhalations which emanate from the body of the patient. The communication of plague by inoculation with the matter of a bubo or other morbid product has been by no means proved; on the contrary, there is every reason to believe that the disease cannot be produced by these means." Vague conceptions such as these are now fortunately only rarely met with in medical literature since inquiries into the etiology of disease are at the present time conducted by methods, and upon lines identical with those that have advanced natural and physical science.

On the 14th of June, 1894, Kitasato of Tokio discovered the specific microbe of plague during an outbreak in Hong Kong (3). Independently Yersin (4) made the same discovery a few weeks later in the same year; and to this micro-organism, the pest-bacillus or Bacillus pestis bubonicœ the name of Coccobacillus pestis has been given by Metchnikoff (5). The majority of bacteriologists regard the bacillus of Kitasato as identical with that of Yersin, but according to Ogata (6), whose researches were made in Formosa during 1896, this is by no means certain, and he adduces among other evidence a statement to that effect made by Kitasato himself. Although it is only to be expected that two original researches on the same subject should exhibit certain differences, which undoubtedly did exist as to the mode of growth, staining, behaviour and motile power of the bacillus, the discrepancies between the two observers are not sufficient to justify the attitude taken by Ogata, Aoyama, Okada and other Japanese observers, which aims at discrediting Kitasato's work. His researches, however, are confined to the actual discovery of the bacillus and the proof of its pathogenic nature.

An excellent account by Kruse in the third edition of *Die Mikro-organismen*, by C. Flügge, contains all that was known of the pest-bacillus in 1895, but since that date numerous papers on this subject have appeared, and the reports of the Scientific Commissions sent to India by most European Governments for the purpose of studying the outbreak of plague in that country have been published,

so that there now exists a large amount of information, not only on the etiology, but also on the modes of communication, means of spread, pathology, and treatment of this disease. In the communication made by Metchnikoff at the International Medical Congress in Moscow, August, 1897, is found an admirable exposition of the nature of the plague (7).

As described by Yersin the specific microbe of plague is a very short bacillus with rounded ends and apparent vacuolation in the centre. The organism, which is not endowed with movement, is often surrounded by a capsule, and Zettnow (8) has demonstrated this by Löffler's method for staining the flagella of bacteria. He considers that the capsule represents the plasma of the bacterial cell, while the microbe itself is a nucleus. By van Ermengem's method Gordon (9) has succeeded in staining a spiral flagellum about twice the length of the bacillus, which is attached to this only at one extremity; occasionally a second flagellum can be seen close to the other. The specimens showing this were obtained from agar cultures of twenty hours' growth at a temperature of 37° C. which had been made from the blood of an inoculated animal. If examined in hanging drops this observer states that the pest-bacilli are to a slight extent motile, a view which is shared by no other worker. With the usual aniline dyes the bacillus stains well, especially at the ends rather than the centre, so that an appearance is presented like the bacillus of chicken-cholera. By the method of Gram it does not stain but becomes tinted if a contrast colour is used. Two characters of the bacillus render it difficult to recognise microscopically, a marked tendency to the production of polymorphic and involution forms, but both these characteristics have been utilised as aids to diagnosis. Klein (10) has pointed out that gelatine cultures twenty-four to twenty-eight hours old show atypical thread-like colonies which resemble those of Proteus vulgaris, and the presence of these colonies which have also been noted by other observers in older cultures he considers may be regarded as characteristic of the pest-bacillus. Hankin and Leumann (11) describe well-marked

and even exaggerated involution forms which can under favourable conditions be seen in twenty-four hours if cultures are made on agar which contains 2.5-3.5 per cent of salt. If kept at a temperature of 37° C. extraordinary involution forms are met with. Large spheres and pear-shaped bodies which often have rod-like extensions are seen, but at later periods of growth these very abnormal forms are not frequently met with. For the success of this method as a test for the bacillus it is necessary to first cultivate the microbe on agar and then transfer it to the salted medium. Haffkine had previously recommended the use of agar having a well-marked alkaline reaction for the rapid production of involution forms, but since it is not possible to keep this medium long at the required degree of dryness, or to obtain such certain and quick results, it is not to be preferred to a salt-agar medium.

The pest-bacillus is easily cultivated outside the body, and indeed grows well on all varieties of media. One of the best of these is the one originally recommended by Yersin, whose statement is confirmed by Wilm (12). It consists of 2 per cent. alkaline peptone solution with the addition of 1-2 per cent. of gelatine. Both on this and in ordinary bouillon made from horse-flesh a culture is obtained which much resembles that of streptococcus pyogenes; flocculent deposits forming at the bottom and sides of the tube, while the rest of the liquid remains clear. At times this liquid becomes diffusely turbid, and these two types of growth in bouillon are produced, according to Abel (13), who worked with a culture derived from Kitasato's original stock and one obtained from a fatal case of plague in London during October, 1896, by the manner in which the medium is inoculated. Neither on Löffler's blood-serum on coagulated ascitic fluid, on gelatine, which is never liquefied by the growth, on glycerine-agar, nor on agar-agar, does the bacillus thrive so well as in bouillon. On the last of the solid media a maximum rate of growth is reached in twenty-four to forty-eight hours at the body temperature, and isolated white transparent colonies with iridescent edges are formed. These colonies differ in size and rate of growth,

the larger ones according to Yersin, show a diminution in virulence similar to that which is seen when examining a growth obtained after successive cultivation. On potatoes the bacillus grows feebly at a temperature of 37° C., and in milk a slight increase can be seen, but no curdling occurs (Abel). No growth at all was seen in milk by Kolle (14), though Wilm has described one accompanied by coagulation. The plague-bacillus grows under both aerobic and anaerobic conditions, develops no gas in media containing sugar, and no indol reaction can be obtained from bouillon cultures. An acid is developed in litmus-bouillon in twenty-four hours, and this in no way can hinder growth, since a degree of acidity such that 10.5 c.c. of decinormal soda was required to neutralise 100 c.c. of an acid bouillon that still remained a good culture-medium. A study of the respiration of the bacillus has been made by Hesse (15). Both the assumption of oxygen and evolution of carbon dioxide show variations, the maximum of which corresponds to the period of most energetic growth and is reached on the fourth or fifth day. A larger volume of oxygen is taken up than the carbon-dioxide liberated, the two processes, like the respiration of all protoplasm, being quite independent of each other. Anaerobic cultures in an atmosphere of hydrogen did not succeed at all on agar, and only to a limited extent on gelatine, showed that a diminished amount of CO_2 was evolved.

When compared with most other pathogenic micro-organisms the bacillus of plague yields rather scanty cultures which require some care for preservation, and it is easily killed by weak solutions of disinfectants like carbolic acid, lyssol, mercury perchloride and milk of lime. The last of these as has been shown by Kitasato, Wilm and others to effectively kill a culture in ten minutes to three hours, according as the added lime varies from .5-5 per cent. in a bouillon culture. Early experiments made by Kitasato have proved that the Pest-bacillus, like many other non-sporing bacteria, is easily killed by drying. The microbes or pieces of tissue from plague-infected animals become non-infective by the removal of water, and the

rapidity with which this is done either in an exsiccator or by exposure to varying temperatures dermines the result. At 29°-31° C. the bacilli are killed in four and a half days, and in three hours if dried over sulphuric acid. When the drying process is slow and carried out in the dark at 16°-20° C. living bacilli are found on the thirtieth day. If in Hong Kong at a temperature of 28°-31° C. four and a half days is needed, then for our climate about ten days would probably suffice to kill the microbe. Direct sunlight and dry heat destroy cultures of the bacillus with certainty, but steam at 50° C. for one hour renders all cultures harmless, and the following table by R. Abel show this to be a most effective means of destruction :—

At 100° C. all bacilli dead at the end of 1 min.
„ 80° „ „ „ 5 „
„ 70° „ „ „ 10 „
„ 60° „ „ „ 20 „
„ 50° „ some alive, some dead at the end of 30 min.
„ 50° „ all dead „ „ 60 „

The pest-bacillus is pathogenic for man and a great number of animals, especially mammals, though the symptoms, progress and termination of the disease are not quite similar in both cases. Gaffky, R. Pfeiffer, Dieudonné and Sticker (25), members of the German Plague Commission recognise three different clinical varieties of the plague. In the true bubonic plague there is a sudden onset of high fever accompanied by characteristic swellings of the superficial lymphatic glands in the groin and the axilla, which form the buboes of typical plague and contain almost pure cultures of specific bacilli with or without the association of pyogenic streptococci. Pest-septicæmia is another form of the disease where the spleen is always enlarged and signs of general sepsis with hæmorrhages into the stomach and bowels are found on autopsy. The third form of plague is Pest-pneumonia in which numerous specific microbes can be recognised in the sputum either alone or associated with diplococcus lanceolatus and pyogenic streptococci. Among human beings the mortality is as high as 80 per cent. After an incubation period which is stated by Lowson (16) to be

about three to six or even nine days, with the spread of the bacilli throughout the body which on *post mortem* are found distributed in blood, lymph, the spleen, marrow, brain, liver, lungs and lymphatic glands, death generally occurs on the third to the seventh day. An excellent clinical history of the plague in Formosa, 1896, together with the macroscopic and microscopic morbid anatomy of cases is given by Yamagiwa (17). A study of the acute hæmorrhagic inflammations in the organs of animals killed by the plague has been made by Honl (18), who speaks of a definite purpura pestica, which is a primary septicæmic condition fatal in two days, and a purpura streptococcica which is a secondary hæmorrhagic infection seen in later stages of the disease. In the spleen and lymphatic glands, by means of sections treated with hæmatoxylin, counter-staining with concentrated aqueous solutions of methylene-blue and subsequent impregnation with tannin, numerous accumulations of zooglœa masses are to be found, in which the separate bacilli are seen separated from each other by a homogeneous envelope. From histological evidence V. Babes and C. Livadite (19) consider the plague-bacillus as a representative type of the group of bacteria that cause specific hæmorrhagic infection in man and animals. They describe and figure distinct hæmorrhagic zones in dilated capillary districts, in which masses of bacteria are seen that have broken through the walls of the vessels and so established innumerable hæmorrhages. They think that is not necessary to regard these as being produced by any bacterial toxin which injures the vessels, or to believe that certain nerve centres are implicated and in this way produce vascular changes, but that the characteristic hæmorrhages are directly produced by the pest-bacilli. It may be pointed out, however, that Charrin, Unna and other observers deny the existence of specific hæmorrhagic bacteria, and, while not disputing the facts, consider that hæmorrhages are dependent on a peculiar disposition found in certain men and animals, and due to such a variety of causes that hæmorrhagic bacteria, in the sense that Babes speaks of these, cannot be ranked as specific micro-organisms.

It is an old observation that the plague which is at first a disease of rats and mice soon becomes a disease of man, and all recent authors confirm this fact. That these animals are really plague-stricken admits of no doubt. In Formosa plague is known as rat-pest, and in Canton one man alone collected 22,000 dead rats during the outbreak in 1894. Animals, as laboratory experiments prove, may succumb either by ingestion of pieces of infective organs or by inoculation at the surface of the body. When passed through a series of animals the microbe, which on cultivation outside the body is easily attentuated, acquires increased virulence, in this respect resembling other bacteria such as those of anthrax and chicken-cholera. H. F. Nuttall (20) has shown that outbreaks of plague in various countries have been described as preceded by or associated with the death of various animals besides rats and mice. Not only these, but also pigs, cats, dogs, snakes, horses, buffaloes, goats, fowls and other birds have been stated to be affected. Experiments conducted by Nuttall (20), Yersin, Wilm, Galeotti and Malenchini (21), Lowson and Devell (22) at the same time restrict and add to our knowledge of the susceptibility of different animals, which after subcutaneous inoculation, die in a few days with symptoms of septicæmic poisoning accompanied by a strong reaction at the seat of the injection. Bubonic swellings of the glands may or may not be present. According to Nuttall's tables the following animals are susceptible: rats, white mice, house mice, shrew mice, guinea-pigs, rabbits, pigs, horses, monkeys, cats, fowls and lizards; while pigeons, dogs, oxen, the tortoise and frogs are immune. One species of the latter (Rana temporaria) is stated by Devell (22) to die of plague if bacterial cultures or pieces of infected organs are introduced into the lymph sac of the animal. A microbe of virulence sufficient to kill mice in two to two and a half days requires thirteen to seventeen days for a frog, but by passage through this animal the virulence of the bacillus is augmented and the time so shortened that death follows inoculation on the fifth day. Many animals which under normal circumstances are immune to bacterial in-

jection, become susceptible when they are starved or fatigued, and pigeons if food is withheld lose their natural resistance to plague (28). Although the pest-bacillus is pathogenic for a large number of animals it is by no means proved that during an epidemic they actually convey the disease, though, of course, this is conceivable. Hankin (23) has shown that there is no necessary connection between infection of animals and outbreaks among men. In many localities near Bombay after the disease had been introduced by human beings, it ran its course without a single rat being affected, and in the neighbourhood of Hurdwar a genuine outbreak occurred among rats and terminated without spreading to the community.

In the bodies of plague-stricken individuals specific microbes can be easily recognised in the glandular swellings, in pus and the blood. Their presence in the latter affords a certain means of diagnosis in doubtful cases, but since comparatively few pest-bacilli are met with in blood a large number of specimens must be made and care taken not to mistake those which are seen for streptococci which are not infrequently present. By blood examination Lowson found the pest-bacillus in 80 per cent. of cases and Wilm in 77 per cent. The latter observer considers that a more certain diagnosis may be obtained by cultivating the suspected blood in bouillon, and has also recommended an examination of the urine which always contains plague-bacilli, not only during the disease but for at least a week after recovery. Two members of the Russian Plague Commission (24) have also shown that the blood serum of patients convalescent from plague has the power of causing an agglutination of the bacilli in a hanging drop or bouillon culture, similar to that originally observed by Chantermesse and Widal when the blood of patients suffering from typhoid-fever was added to cultures of bacillus typhosus or the serum of cholera to cholera microbes. This phenomenon of agglutination in cases of plague is not seen during the first week, but appears by the seventh day, persists until the fourth week and then finally is lost. The members of the German Plague Commission (25) have also noted this re-

action which enables the pest-bacilli to be recognised with certainty.

Infection by the bacillus in the majority of cases is at the surface of the body, minute wounds and abrasions of the skin affording a means of entrance. More rarely the mucous membranes of the intestine are primarily affected, and though Wilm believes the majority of cases in Hong Kong were infected in this way, the reports of the various commissions in Bombay state that this, though it may occur, is rarely seen. Experimentally animals may be infected at the intestinal or respiratory surfaces and pest-bacilli are frequently found in sputa and always in the excreta. Lesions in the mouth or tonsils are also sites where the bacilli may obtain an entrance into the body, but apparently these never primarily infect by the mucous membrane of the stomach (24). The cells lining the alimentary canal undoubtedly oppose the entrance of bacteria. Most careful observations recently made by Neisser (26) show that chyle and blood are absolutely free from germs even after the liberal ingestion of pathogenic bacteria. The mesenteric glands remain normal, and it is only when the mucous membrane is diseased that bacteria can leave the cavity of the intestine, and even then these are not absorbed, but grow through the wall of the bowel in a manner comparable to the growth which is known to take place through the substance of a filter. Wilm and Abel have both examined the behaviour of pest-bacilli in water, since it is conceivable that this may be a vehicle for transmission of the specific germs. The former observer found bacilli lived for twenty days in distilled water, in spring water sixteen, and in sea water six days, 200 c.c. of water being mixed with .5 c.c. of an agar culture. Abel's researches also show that the specific microbes of plague can live for weeks in water, so that by washing in this there will be a possible risk of infection. Gaffky and his colleagues however have shown that in absolutely sterile water the bacilli live only three days and in ordinary tap-water for one day, so that plague cannot be considered to be a water-borne disease.

The part played by insects in the spread of plague has

been studied by Nuttall (20). It had been already noticed by Yersin (4) that flies died of plague, and that an infected insect could establish the disease in a guinea-pig. Many flies, however, die of water, and the laboratory experiments by Yersin have been greatly extended by Nuttall's researches, who has worked out this subject with the greatest care, making use of cultures from Kolle and A. Macfadyean of London, which were sufficiently virulent to kill mice in thirty-six to forty-eight hours. To any one familiar with the swarms of flies in the East, it would appear very probable that if these insects can be infected, which undoubtedly is the case, they might be an efficient means in spreading the disease. In all experiments to decide this question control experiments were devised. Flies fed with infected bouillon died much earlier than those in the control apparatus, and their bodies contained pest-bacilli which were pathogenic for mice. Infected flies which had not died contained virulent bacilli for forty-eight hours and longer, after they were removed from the apparatus. Similar experiments were made with bugs, but in these insects the bacilli gradually die. In the case of mosquitoes and fleas no experimental evidence exists, though it is a well-established fact that the former can infect man with filariœ, and virulent pest-bacilli have been found in fleas taken from plague-stricken rats (25). Hankin has observed that ants rapidly eat up rats dead of plague, and their excreta contains virulent bacilli. Ants, however, do not die of plague, nor do they retain the infection for any length of time (27). From a practical point of view, though the actual proof in any epidemic has not yet been given, it cannot be denied that flies at any rate may play a part in spreading the plague, and at any rate may contaminate food or water by their excreta.

Like other pathogenic bacteria the bacillus of plague produces toxins, which accumulate in the micro-organisms and are discharged by these into the surrounding medium. The fever of plague is directly due to these products. Lustig and Galeotti (29) have obtained various products by extracting cultures of the microbe for twelve to twenty-four

hours at a temperature of 10°-12° C. with .75 per cent. potassium hydrate. This treatment yields a viscid opalescent fluid which is filtered through paper, and the precipitate obtained from the filtrate by precipitation with weak acetic acid is collected, washed, dried and dissolved in sodium carbonate. Prepared in this way the solution probably contains among other bodies a nucleo-proteid, and is found to be toxic for rats and mice. The same substance if injected in non-lethal doses, either subcutaneously or into the peritoneal cavity, confers immunity to infection by virulent pest-bacilli, and this condition lasts for about four weeks, and the serum of the blood of these animals during this time possesses both an immunising and protective power. In the method devised by Roux the pest-bacillus is rendered exceedingly virulent by introducing microbes enclosed in small collodion bags into the peritoneal cavity of the rabbit. Within the sacs a free development takes place, and the bacilli acquire a high degree of virulence. If these are now grown upon gelatine bouillon the toxic products of the bacteria can be separated by filtration through a Pasteur-Chamberland tube. A still more active toxin is obtained if the bacilli are allowed to die in the culture fluid, which then extracts the toxin. The clear fluid rich in toxin is precipitated by ammonium sulphate, and the deposit collected and dried. The activity of this powder is such that .25 milligrammes will kill mice in a few hours. There is nothing of special interest connected with the toxin of the plague-bacillus. It is obviously a mixture of several substances, some of which are of a proteid character. The chemistry of this, as of other toxins, is a matter of conjecture, and this is recognised only by its effects, for there is no doubt that by small repeated injections a state of immunity can be conferred on animals otherwise susceptible to infection with living plague-bacilli.

The prophylactic and curative power of the serum of animals that have been experimentally brought to a high grade of acquired immunity by the introduction of toxins, dead bacilli or attenuated cultures into the system has been extensively studied by Yersin (31), Haffkine (32),

Calmette and Borrel (33), Galeotti and Malenchini (34), Wladimiroff (35) and others, and the members of the several Plague Commissions in Bombay have furnished long reports on the value of the serum treatment in plague as a preventive and therapeutic measure. In Haffkine's method, which is similar in principle to that practised by him as a protection against cholera and typhoid, a fluid preparation obtained from dead cultures of pest-bacilli is employed; 2.5 c.c. is injected into the forearm of an adult; fever of slight amount accompanied by pain and swelling at the seat of injection are the chief phenomena of the reaction. This treatment affords considerable but not absolute protection, and its beneficial effect tends to disappear. Yersin, Calmette and Borrel consider filtrates of cultures which contain toxin are useless for protective purposes. By employing the serum of horses that have been immunised by subcutaneous injection of cultures of the pest-bacillus warmed to 58° C., mice and other animals can both be protected from and cured of plague. That serum so prepared has specific anti-toxic power appears to be proved, since the serum of normal horses, the anti-diphtheritic serum of Roux, the antitetanic serum of Roux and Vaillard, the antivenin of Calmette and the antistreptococcic serum of Marmorek, were all found to be useless as a means of protection against an infection by virulent plague-bacilli. An analysis of cases treated by Yersin's serum shows that while the mortality in hospitals is 80 per cent. of those attacked, this number is reduced to 49 per cent. By experiments made on monkeys (Semnopithecus entellus the grey, and Macacus radiatus the brown ape, the latter of which is remarkably susceptible to plague) Galeotti and Malenchini have established the therapeutic and protective value of the serum of horses that have been rendered immune by the repeated injection of small amounts of toxin.

With the development of bacteriology not only have the specific forms that determine disease been isolated, but the progress of this science has shown all civilised countries the way to guard against the introduction of such a scourge as plague, which in former centuries half depopulated the large

cities of Europe. The therapeutic use of antitoxic sera, which is admitted by all who are competent to form an opinion to be of the greatest use in tetanus, diphtheria and snake-bite, is the direct outcome of bacteriological work, and the services rendered by this science to practical medicine in such a disease as the plague can hardly be overestimated.

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G. A. BUCKMASTER.

SECRETION AND ABSORPTION OF GAS IN THE SWIMMING-BLADDER AND LUNGS.

PART I.—SWIMMING-BLADDER.

IN the present paper I propose to give a short account of what has been ascertained with regard to the conditions which determine separation and absorption of gas by the swimming- or air-bladder. The subject is one of considerable interest, since it seems to lead up very directly to certain questions of fundamental importance in biology.

The air-bladder, as is well known, is an elongated sac containing gas, and usually lies dorsal to the alimentary canal, of which it is an out-growth. It is present in most, but not in all, fishes. The duct communicating with the alimentary canal is in some species permeable, and in others completely closed.

It has been generally assumed for long that the chief function of the air-bladder is to enable the animal to increase or diminish its specific gravity, and thus alter the depth at which it swims. Borelli in his famous book, *De Motu Animalium*, says that when it wishes to go down, the fish, by means of its muscles, compresses the air-bladder, and thus diminishes the volume and correspondingly increases the specific gravity of its body. When the fish wishes to rise it relaxes its muscles, and thus diminishes the specific gravity.

Another theory which deserves mention is that of Delaroche,[1] who supposed that the muscles which compress the swimming-bladder are in a state of tonic contraction, regulated according to the depth at which the animal wishes to remain. When the animal rises or goes down the contraction becomes more, or less, vigorous, so that the specific gravity is kept constant. The function of the air-bladder is thus to enable the animal to remain at any desired depth in the water.

The whole subject of the functions of the air-bladder was carefully investigated about twenty years ago by

[1] *Annales du Muséum d'histoire naturelle*, 1809.

Moreau,[1] whose work, however, does not seem by any means so well known as it ought to be. He proved that the current ideas about the mode of action of the air-bladder were entirely erroneous, and that a fish in reality makes no use of its muscles in regulating the volume of its air-bladder. When the fish descends, the air in the bladder is compressed and the specific gravity of the body increases, so that it naturally tends to sink farther and farther, and if it got too far would sink to the bottom. Conversely, if the fish ascends its air-bladder expands, and if it goes too far it is helplessly carried to the surface, unless it possesses a permeable air-duct or the air-bladder bursts outwards. Thus

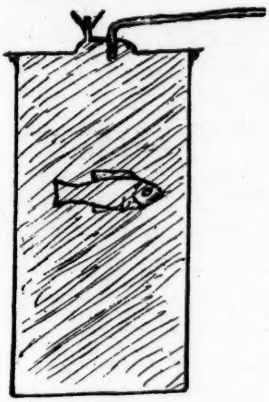

FIG. 1.

the air-bladder of a fish tends to make it behave exactly like the toy known as the "Cartesian Diver". From this point of view it would seem as if an air-bladder must be a source of great inconvenience to a fish, exposing it to a constant danger of being carried helplessly to the surface or to the bottom. Its position at the best is evidently one of unstable equibibrium, which the smallest movement upwards or downwards could disturb.

Moreau demonstrated the facts just mentioned by means of several experiments, one or two of which may be described. He placed the fish in a large and tall glass vessel completely filled with water (Fig. 1). Into this vessel

[1] *Mémoires de Physiologie*, Paris, 1877.

there was fixed at the top a glass tube, with a horizontal arm. Any change in the volume of the fish was of course indicated by a movement in the liquid in this tube. He found that as the fish swam upwards its volume expanded, as shown by the movement of the water in the horizontal tube, and conversely as it swam downwards. The movements of the water in the tube always corresponded exactly to the height of the fish, just as if the latter had been a "Cartesian Diver," pulled up and down by a thread. Similar results were obtained when, by means of slightly different experimental arrangements, the pressure

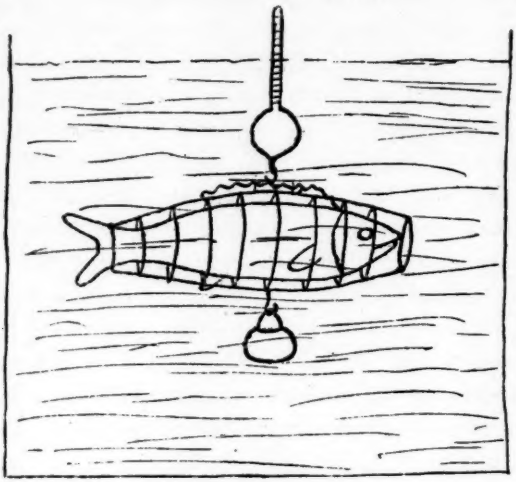

FIG. 2.

inside the vessel was artificially raised or lowered. Thus the fish made no use of its muscles to compress its air-bladder. Compression of the air-bladder, when it did occur, seemed to be merely accidental and momentary, as when the animal made a sudden and violent movement. Even with a species possessing a specially muscular swimming-bladder the result was precisely the same. If the fish was confined in a closely fitting wire cage, so that it was unable to move its tail or fins (Fig. 2), or if it was anæsthetised with ether, it behaved, when subjected to differences of pressure, exactly like a "Cartesian Diver".

Moreau's further experiments showed that the air-bladder may in reality so act as to assist the animal in balancing itself at any required depth, but that the action is an exceedingly slow one, and not dependent in any way on the muscles. He took a fish which had been living at the surface and determined its specific gravity by means of the arrangement show in Fig. 2. He then sank the fish in a cage to a considerably greater depth, and after about two days pulled it up again, and redetermined its specific gravity at the same pressure as before. He always found that the fish was now much lighter. It had gradually secreted air into its air-bladder while at the deep level, so that at last its specific gravity corresponded with that of the water. On coming up again the extra air of course caused it to swell, so that it was much lighter when brought back to its old level.

When the air-bladder was artificially emptied, either by puncture with a trocar, or (in the case of fish with a patent duct) by producing a vacuum in the vessel containing the animal, the immediate result was that the fish sank helplessly to the bottom. After a few hours, however, it was able to swim about again as usual, gas having gradually been secreted into the air-bladder. This experiment may very conveniently be performed on a goldfish. All that is necessary is to place the animal in a large-sized bottle provided with a perforated rubber cork, through which passes a tube connected with a filter-pump. As soon as sufficient air has escaped from the swimming-bladder the tube is removed. The fish at first remains helplessly at the bottom of the bottle, but gradually recovers, and next morning will be swimming about again as usual. Another experiment described by Moreau is to take two fishes, and attach a float to one and a sinker to the other. At first the fish with the float is unable to leave the surface, while that with the sinker is unable to leave the bottom. After some hours, however, the two fishes will be swimming about together as usual, the fish with the float having absorbed some of the gas in its air-bladder, while the fish with the sinker has secreted gas. This experiment seems to indicate that the

secretion and absorption of gas is in some sense under voluntary control. On increasing or diminishing the pressure within a large vessel of water in which a fish is swimming, it is easy to see that the animal by swimming downwards or upwards endeavours to counteract the tendency of the expansion or compression of the air in the swimming-bladder to carry it in the opposite direction. The fish is evidently made very uncomfortable by alterations in the volume of its air-bladder, and the discomfort causes it to assume the depth at which the volume of the air-bladder is normal. An animal swimming in the sea will thus tend to be kept at about one depth. Permanent alteration of depth will at any rate only be brought about gradually. The air-bladder thus seems to be a contrivance of such a nature as to prevent a fish from suddenly altering, except to a very limited extent, the depth at which it lives. Presumably it is of advantage to the animal to remain at about one level; on the other hand, gradual changes of depth will be compensated for by secretion or absorption of gas, so that if the change is only made gradually enough the animal will be able to live equally comfortably at the most varying depths. Moreau remarks that flat fish have no air-bladders, and this is what might be expected from the fact that they rest on the bottom on their sides.

I now come to the facts with regard to the air-bladder which are of the chief interest to physiologists. Early in the present century it was found by Biot [1] and other observers that the gas in the swimming-bladder is not air, but a variable mixture of oxygen and nitrogen, together with about 1 or 2 per cent. of carbonic acid. The most remarkable fact discovered about this mixture, however, was that it frequently consisted almost entirely of oxygen, especially in deep-sea fishes.

The following table gives the results of a number of analyses by Biot:—

[1] *Mémoires de la Société d'Arcueil*, 1807.

BIOT'S ANALYSES OF GAS FROM THE AIR-BLADDER.

Name of Fish.	Depth at which it was caught.	Percentage of oxygen in the air-bladder.
Mugil cephalus - -	Close to surface	Traces
Mugil cephalus - -	" "	"
Sparus annularis ♂ -	" "	9
Sparus annularis ♀ -	" "	8
Sparus sargus ♂ -	43 feet	9
Sparus sargus ♀ -	" "	20
Holocentrus marinus -	" "	12
Sparus melanurus -	Close to surface	20
Labrus turdus - -	12 feet	16, 24 and 28
Labrus turdus - -	43 "	24
Sciaena nigra - -	" "	27 and 25
Sparus dentex - -	120 "	40
Sparus argenteus -	390 "	50
Holocentrus gigas -	Great depth[1]	69
Gadus merluccius -	" "	79
Trygla lyra - -	" "	87

By Johannes Müller and other physiologists this gas was spoken of as a secretion, but ideas about secretion were then still in their infancy. Müller, it is true, possessed what appears to be the right idea, that all true secretion is, in the most intimate sense, a "vital process," and that therefore the problem of the cause of secretion directly involves the whole question of the cause of life itself. This idea of Müller's was, however, for the time lost sight of in the course of the movement towards physico-chemical speculation which overwhelmed physiology about the middle of the present century.

Biot draws attention to the fact (evident from the table of his analyses) that the greater the depth at which a fish is caught the higher the percentage of oxygen in its air-bladder. Moreau confirms this; and his experiments throw further light on the subject. He found that if the air-bladder was artificially emptied, either by puncture or by reduction of atmospheric pressure, the gas which is afterwards gradually secreted into the bladder is richer in oxygen than before, and usually far richer in

[1] Often caught at 3000 feet.

oxygen than air. It would seem therefore that pure, or nearly pure, oxygen is secreted into the bladder. A similar result is obtained by sinking a surface fish into deep water in a cage. As much as 90 per cent. of oxygen may be present in the gas from the bladder.

It is generally assumed that the passage of gases throughout the body, and between the body and its environment, is due simply to diffusion. This theory is probably perfectly satisfactory for many cases, but does not account for the facts connected with the swimming-bladder. Supposing the bladder to have been originally filled with air, which had passed in, it might be, through the duct when the fish was young, what would happen to this air on the diffusion theory? In the first place its oxygen percentage would never increase. The amounts of oxygen and nitrogen dissolved in sea water depend on the tensions or partial pressures of each of these gases in the atmosphere, and on their co-efficients of solubility. Sea water is known to be saturated with nitrogen, and more or less saturated with oxygen at the pressures which these gases respectively exercise in air. Supposing some of this gas to come in contact with an atmosphere of gas in which the partial pressure of nitrogen or oxygen was less than in air, then nitrogen or oxygen would be given off from the water until a state of equilibrium was again established. Conversely, if the partial pressure of nitrogen or oxygen in the gas was greater than in air the water would take up these gases. The air and the water have the same gas-tensions when the equilibrium is established. Two liquids, such as sea water and the blood passing through the gills, will by simple diffusion tend to assume the same oxygen, nitrogen and carbonic acid tensions. On the diffusion theory the blood of course cannot have a higher oxygen tension than the air, hence it cannot give up oxygen to the air-bladder if the latter already contains oxygen at atmospheric tension. Actually, however, the oxygen tension of blood passing through a tissue such as the wall of the air-bladder will be less than that of air, owing to the using up of oxygen for respiratory purposes. Hence the blood will absorb oxygen

from air in the air-bladder. The consequence of this will be that the nitrogen percentage in the air-bladder will be increased, and as the total pressure within the air-bladder is at any rate not less than that of the atmosphere, the partial pressure or tension of the nitrogen in the air-bladder will be greater than that of air, and therefore greater than the nitrogen tension of the blood. The nitrogen of the air-bladder will therefore follow the oxygen, and finally, there will be no gas of any sort left. If the fish is much below the surface the process of absorption of gas from the air-bladder will be far more rapid since the partial pressures of its gases will then be far above the partial pressures of the corresponding gases in the atmosphere or in the water. Every thirty feet of depth will increase by about an atmosphere the pressure of the gases in the air-bladder. Thus on the diffusion theory all the gas ought to disappear from the air-bladder, whereas actually the gas in it remains unabsorbed, or may even increase.

When gas is from any cause introduced into any closed space in the body, such as the pleural cavity or the blood-vessels, absorption does actually occur, and this absorption is rendered intelligible by the explanation just given on the diffusion theory. The same explanation also applies to the well-known fact that when the Eustachian tube is blocked by catarrh a partial vacuum tends to be produced in the middle ear, causing deafness from collapse of the membrana tympani, etc.

When the fish goes far below the surface of the water the pressure in the air-bladder of course increases enormously. Hence the partial pressure of one or all of the gases in the air-bladder rises high above the partial pressure of the same gases in the water, particularly in the case of oxygen, when the bladder contains a high percentage of the latter gas. Consequently, on the diffusion theory, these gases ought to be rapidly absorbed. At the great depths at which some fish are caught, the partial pressures of the gases in the air-bladder are enormous. To take an example, in the case of a fish (Synaphobranchus pinnatus) caught at a depth of 4500 feet, the gas from the air-bladder

was found by Schloesing and Richard[1] to contain 85 per cent. of oxygen and 12 per cent. of nitrogen and argon. The pressure at this depth is about 150 atmospheres, hence the tension of the oxygen in the bladder was about 127 atmospheres, and of the nitrogen about 18 atmospheres, as compared with tensions of a fifth and four-fifths of an atmosphere respectively in the sea-water. At such a pressure complete absorption of the gas ought, on the diffusion theory, to occur with the utmost rapidity, whereas nothing of the sort happens, and these fishes must be able to secrete oxygen and nitrogen against these enormous pressures. The fact is the more remarkable since oxygen at anything more than 4 or 5 atmospheres' pressure is exceedingly poisonous to both animals and plants. Yet the wall of the air-bladder is not susceptible to the poison. In this respect it may be compared with the wall of the stomach, which, so long as it is alive, resists the action of the exceedingly poisonous hydrochloric acid which it secretes. In Dolium Galea (or water snail) a digestive juice containing 4 per cent. of sulphuric acid is secreted, yet this corrosive liquid, which would instantly kill almost any other living tissue, does no harm to the glands which secrete it.

The gas secreted by the walls of the air-bladder seems usually to be a mixture of oxygen and nitrogen. The fact that with increasing depth the oxygen percentage as a rule increases would seem at first sight to suggest that oxygen alone is actually secreted, the nitrogen simply diffusing in until its partial pressure equals that of the nitrogen in the water. This hypothesis is however not tenable. The example quoted above shows that not only the oxygen tension, but also the nitrogen tension in the air-bladder, may far exceed that of the water. In some fishes, moreover, the gas contained in the air-bladder may be pure, or almost pure nitrogen. Thus in Coregonus Acronius, a freshwater fish living at a depth of 200 to 250 feet in the Lake

[1] *Comptes Rendus*, vol. cxxii. (1896), p. 615. The writers suggest that the oxygen found by them may have been given off from the hæmoglobin of the blood while the animal was being drawn to the surface. Evidently, however, this cannot be the case.

of Geneva, Hüfner found that the gas consisted sometimes of perfectly pure nitrogen,[1] which must have had a tension of six or seven atmospheres. Schloesing and Richard found that the "nitrogen" from the air-bladder of Synaphobranchus pinnatus contains about 1·94 per cent. of argon. The argon tension in the air-bladder at the depth at which the animal was caught must thus apparently have amounted to 35 per cent. of an atmosphere. Since the argon tension of sea-water is presumably only ·93 per cent. of an atmosphere it would thus seem as if even argon may be secreted by the swimming-bladder.

Moreau noticed that section of the sympathetic nerve fibres going to the walls of the air-bladder seems to hasten the secretion of gas into the empty bladder, and more recently Bohr[2] has found that section of the vagus branch entirely stops it. Thus the secretion of gas, like secretion from the salivary or other glands, is under the control of the nervous system.

It has been known for long that in the walls of the air-bladder of many fishes there are present "retia miralulia," *i.e.*, bunches or discs of finely divided vessels. These structures are, however, beneath the lining epithelium, and it is difficult to see how they can have any very direct connection with the secretion of gas. The epithelium, however, is often differentiated into a more or less gland-like structure (the "epithelial body") which was described a few years ago by Coggi. Anything like full and complete investigations of these structures do not seem to have been made yet, although presumably the structures described by Coggi are real air glands.

As regards the process by which gas is secreted it is difficult to avoid the conclusion that the molecules of gas are liberated from some form of combination within the cells lining the air-bladder. It seems not unlikely that this process is continually going on, even while the actual amount of gas in the bladder is not increasing. Moreau

[1] *Archiv für Anatomie u. Physiologie*, 1892, p. 54. The argon was, of course, not determined.

[2] *Journ. of Phsiol.*, vol. xv., p. 494.

has shown that when a fish is asphyxiated it quickly uses up the oxygen in its air-bladder. Evidently therefore the oxygen may pass outwards into the blood, and considering the physical properties of the air-bladder it is difficult to believe that diffusion outwards is not constantly occurring to a certain extent.

If the gas is liberated from combination within the cells, then we have in the animal kingdom a process which may be compared to the liberation of oxygen from the green parts of plants, and the fixation of free nitrogen by the parasitic organisms in Leguminosæ. Life is commonly regarded as being essentially an oxidation process, the activity of the green parts of plants in presence of light being looked upon as something exceptional, directly due in some way to the presence of a specialised pigment. The fact that free oxygen may be liberated even in the case of animals must tend to shake this belief, which in any case is rendered difficult by the fact that the only plausible theory to account for the presence of free oxygen in our atmosphere seems to be the presence of living organisms capable of liberating oxygen. Organisms capable of liberating oxygen must thus apparently have preceded in order of development those living on free oxygen. Physiologists in recent times have been apt to regard life too much from a physical and chemical, and too little from a biological, point of view, and it seems possible that just as morphological identity is concealed under the greatest diversities of physical form, so physiological identity is concealed under great diversities of physical and chemical process, and that comparative physiology, working by means of biological and not merely physical and chemical conceptions, will fill up the apparent gap between physiological processes which in a chemical sense are as widely separated from one another as those of oxidation and liberation of free oxygen.

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